The Institute of Physics

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The Bulletin is published by The Institute of Physics for circulation to its members.

Notices to Members

Dr. H. R. Lang : Twenty-five years' service

Dr. Lang has completed twenty-five years as Secretary of the Institute, having been appointed on January 1, 1932. At the meeting of the Board on May 7 the President spoke warmly of Dr. Lang's great contribution to the development of the Institute and the widespread appreciation that his work has aroused. As a mark of recognition of his outstanding service for a quarter of a century the Board have presented to Dr. Lang a Parker-Knoll armchair.

Contributions to the Bulletin

Although there is a large amount of material in hand for publication in the *Bulletin*, the average delay being about six months at present, the Editor is always glad to consider contributions from members, in the form of either short articles or correspondence. Articles of less than 2,000 words would be particularly welcomed. They may be on any topic likely to be of wide interest among members and not so predominantly technical in nature that they would be more suitable for publication in one of the Institute's scientific journals. All articles submitted are subject to refereeing. Manuscripts should be addressed to the Deputy Secretary.

Scientific Education of Physicists

A new edition of the very successful publication *The Scientific Education of Physicists* is now available. Copies may be obtained, free of charge, from the Deputy Secretary.

National Certificate Examination Papers

A selection of question papers set by various colleges in the 1956 examinations for

Ordinary and Higher National Certificates in Applied Physics has been published. The papers should be of use to teachers in technical colleges and to any physicists whose assistants are students or potential students for the courses for these Certificates. Copies are available from the Deputy Secretary, free of charge.

British Conference on Automation and Computation

At the invitation of The Institution of Civil Engineers, The Institution of Mechanical Engineers and The Institution of Electrical Engineers, exploratory Conferences were held on March 6 and April 16, 1957, as a result of which the representatives of some twenty organizations, with interests in the fields of automation and computation, approved proposals for closer collaboration, and agreed to recommend them to their Councils for adoption. The proposals, if accepted by the bodies concerned, will result in the setting up of a British Conference on automation and computation which will be organized in three groups of Societies, having the following fields of interest :

Group A.—The engineering applications of automation techniques ;

Group B.—The development and applications of computers, automatic controls and programming techniques ;

Group C.—The sociological and economic aspects of automation and computation procedures.

The following bodies would be invited to act as the conveners and sponsors of the three groups.

Group A : The engineering applications of automation techniques.

Convener : Institution of Mechanical Engineers ; *Additional Sponsors :* Institution of Civil Engineers, Institution of Chemical Engineers, Institution of Electrical Engineers, Institution of Production Engineers.

Group B : The development and applications of computers, automatic controls and programming techniques.

Convener : Institution of Electrical Engineers; *Additional Sponsors :* Institute of Cost and Works Accountants, Institute of Physics, Office Management Association, Society of Instrument Technology.

Group C : The sociological and economic aspects of automation and computation procedures.

Convener : British Institute of Management *Additional Sponsors :* Industrial Welfare Society, Institute of Cost and Works Accountants, Institute of Personnel Management, Institution of Production Engineers.

An essential feature of the proposals is that liaison between the groups should be established on a permanent basis from the outset, and it is foreseen that the central unit of the Conference which will be established for this purpose will undertake such other duties as may be deputed to it by the groups ; for example, the preparation of periodical digests of published papers falling within the general fields of automation and computation.

The Board of The Institute of Physics has accepted the proposals and appointed Dr. A. D. Booth and Dr. H. R. Lang as its representatives to Group B ; they were its representatives at the exploratory Conferences.

Harwell Reactor Technology Course

Reactor technology courses specially arranged for senior technical executives will be held at the Reactor School, Atomic Energy Research Establishment, Harwell, from 21st to 31st October, 1957, and from 6th to 16th January, 1958.

Lectures will cover the physical, metallurgical and engineering problems involved in the design of power reactors and the chemical problems of fuel and effluent processing. There will also be visits to laboratories and reactors within the Establishment.

The fee for a course is £50 (excluding accommodation). Selection of the students will be made three months before the start of each course.

Requests for information about these and other reactor technology courses should be addressed to : The Reactor School, Atomic Energy Research Establishment, Harwell, Berkshire.

Teachers for Technical Colleges

IN September, 1956, the Minister of Education appointed a Committee "to consider in the light of the White Paper on Technical Education (Cmd. 9703) the supply and training of full-time and part-time teachers for technical colleges, and to make recommendations." The Committee has now published its report: *The Supply and Training of Teachers for Technical Colleges* (Her Majesty's Stationery Office: Price 4s.). Among the recommendations are the following, the paragraph numbers being those in the list of recommendations on pp. 65-68 of the report.

1. Full-time staff in technical colleges should be increased from 11,500 in 1955-56 to 18,600 in 1960-61; and part-time staff from 39,000 in 1955-56 to 47,000 in 1960-61,
2. In order to achieve the total of 18,600 full-time teachers in service by 1960-61, average annual recruitment between 1955-56 and 1960-61 should be increased from the present 1,300 to 2,300; in particular, recruitment of science and mathematics graduates should be increased from 300 to 400 and of graduate technologists from 150 to 360,
5. More use should be made of part-time teachers drawn from industry and government departments for day work. In particular senior members of industrial staffs should be given a special status in the colleges, permitting them to be closely identified with all the academic activities of the colleges. A title such as "special lecturer" would be appropriate.
7. Indefinite deferment from National Service should be extended to graduates with second class honours in science and technology.
12. In order to assist the transfer of people from industry to full-time teaching, industrial pension schemes should, wherever possible, provide for paid up policies to be awarded to employees who resign for this purpose.
13. Every effort should be made to reduce the excessive amount of actual teaching at present required of many teachers.
15. More generous provision should be made of clerical, laboratory and research assistants,
17. Teachers should be given greater encouragement and assistance to return to industry or commerce to refresh their experience. The existing arrangements for release should be amended to allow the teacher to retain any payments made to him in industry. College establishments should be adequate to permit such release; advantage might sometimes be taken of the absence of students during the works period of sandwich courses.
21. Requests for leave to attend scientific conferences should be considered sympathetically and financial help with expenses should be given in suitable cases.
25. A period of training should be encouraged particularly for teachers concerned with the less advanced levels of work, Varied training facilities should be provided for this purpose,
31. A residential staff college should be established to enable senior college staff to study teaching, administrative and technical problems in conjunction with representatives of industry, universities, government establishments and schools. A governing body and a director of studies should be appointed at once,
36. As an incentive to undergo training, teachers who have completed a one-year full-time course of pre-service or in-service training should receive, during service as Assistant Grade A or B, an addition to salary not paid to untrained teachers.
37. An addition to their fees should be paid to part-time teachers who have gained the Technical Teacher's Certificate of the City and Guilds of London Institute or a similar qualification.
46. The Minister should be recommended to set up a standing advisory committee on the supply and training of teachers for further education.

The Institute was among the bodies invited to submit its views for the consideration of the committee and the following letter was sent on behalf of the Board:

"The Board of The Institute of Physics welcomes the appointment of a committee to consider the supply and training of teachers for technical colleges; it agrees that the effectiveness of plans to expand the provisions for technological education in this country will depend critically upon the ability of the Government to recruit good, well-qualified teachers in adequate numbers. This is not a problem, however, upon which The Institute of Physics is in a position to submit detailed evidence for the consideration of the committee, but it hopes that the following general considerations will be borne in mind.

"The supply of scientists and technologists for industry and Government service

will continue to depend upon the provision of adequate numbers of good teachers in the universities and schools as well as in the colleges of technology, and any increase in the number of technical teachers which is made at the expense of the schools or the universities will defeat the aim of the Government as expressed in the recent White Paper. In the past few years technical college teachers of physics, and perhaps of other pure sciences, have too often been recruited from grammar school teachers of the subject, with a consequent impoverishment of the basic training of the students entering universities and colleges and, it is feared, some reduction in the number of such students.

" It is, moreover, highly desirable that the technical college teacher of basic science should have experience in industry, in research or in some other relevant field. The recruits to teaching who come from industry or these other scientific occupations should, however, be men who have achieved some success in this work rather than, as has sometimes happened in the past, men who have failed to make any mark in research and have thereupon turned to teaching. If these recruits are to be of the right quality for advanced work in their subject, only the more senior teaching grades would carry salaries sufficient to attract them. Colleges might, therefore, be encouraged to regard vacant senior posts as a means of attracting well-qualified young men with a few years of industrial or research experience, rather than as opportunities of internal promotion of men with wholly teaching experience. Regular and widespread arrangements should also be made with industry whereby teachers could obtain necessary experience in industrial laboratories. The Board considers that it might also be possible to obtain the co-operation of industry in schemes that permitted industrial physicists to undertake day-time teaching in their special subjects on a part-time basis.

" Among the factors that might affect recruitment to technical colleges adversely are the generally applying teaching loads, which are considerably heavier than those of university staffs, the inadequate opportunities for suitably qualified and interested members of staff to carry out research, and a generally rather illiberal attitude, when contrasted with industrial practice, towards the natural desire of the best teachers to keep abreast of their subjects by attendance at conferences and scientific meetings. Except for the man with industrial experience, referred to above, it is unlikely that the present level of salaries in technical colleges would dissuade potential technical teachers ; indeed there is reason to believe that for the young man the salaries available in technical colleges are comparatively attractive. A further difficulty affecting the recruitment of young teachers is the obligation of National Service. Now that it is possible for a limited number of graduates to fulfil their National Service obligations by becoming teachers in schools, other educational establishments—including colleges of technology and the universities—must be at a disadvantage in recruiting young men to their staffs so long as the concession does not apply to them also.

" It might be desirable to encourage more recruits to technical teaching to follow courses of training in teaching methods, but this point should be approached with caution. It would be unfortunate if the otherwise well-qualified scientist or technologist were to be placed at a disadvantage, financial or otherwise, because of the lack of a training qualification.

" The committee will, of course, recognize that so long as the number of competent scientists and technologists remains below that needed to satisfy the requirements of all employers of scientific manpower, an increase of the number engaged in any one field can be gained, temporarily at least, only at the expense of the other scientific occupations. Any effective solution of the immediate problem concerning teachers must therefore take cognizance of its possible effects upon other scientific work."

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The Theory of Practical Joking — Its Relevance to Physics*

By Professor R. V. JONES
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AT FIRST SIGHT there may seem little relation between physics and practical joking. Indeed, I might never have observed their connection but for an incidental study of the life of James Clerk Maxwell. Two things, among many others, struck me. The first was the growth of his sense of fun from the primitive joke of the boy of six tripping up the maid with the tea tray to the refined, almost theoretical, jokes of his later life. The second was his mastery of analogy in physical thinking : already, at the age of twenty-four he had written a part-playful, part-serious essay on the theory of analogy which showed two of the main features of his mind. On the lighter side, he pointed out the relation between an analogy and a pun : in the former one truth lies under two expressions, and in the latter two truths lie under one expression. Hence from the theory of analogy one can by reciprocation deduce the theory of puns. To the more serious side of Maxwell's understanding of analogy I shall return later, but all this set me thinking about the possible connection between the theory of practical joking and physics. One factor which encouraged me was the high incidence of mischievous humour among physicists. Even Newton, it is recorded, caused trouble in his Lincolnshire village as a boy by flying at night a kite carrying a small lantern ; and in this century the spritely skill of the late Professor R. W. Wood and Professor G. Gamow is already legendary. While I hope to illustrate this paper with examples, I propose first to analyse (if this is not altogether too brutal a process) the essentials of a joke.

INCONGRUITIES

The crux of the simplest form of joke seems to be the production of an incongruity in the normal order of events. The schoolboy laughs, for example, at the sight of his master slipping on a banana skin because this is distinctly abnormal behaviour on the part of one whom the boy regards as dignified. And when we contemplate such a scene we can see that there may indeed be something in the psychologist's theory that laughter had its origin in shouts of triumph over a fallen enemy. Now in most jokes the incongruity is neither as simple nor as violent as this one ; and it is not necessary that it should cause pain. Hence, if we follow the career of an individual joker who is also a man of feeling, we find that his jokes grow more refined and less painful, while the incongruities mount.

We hear the story, for example, of Maxwell showing Kelvin some optical experiment, and inviting Kelvin to look through the eyepiece. Kelvin was surprised to find that, while the phenomenon described by Maxwell was undoubtedly there, so was a little human figure, the incongruity, dancing about. Kelvin could not help asking " Maxwell—but what is the little man there for ? " " Have another look, Thomson," said Maxwell, " and you should see." Kelvin had another look, but was no wiser. " Tell me, Maxwell," he said impatiently, " What

* Based on a lecture given to the Midland Branch of the Institute on February 12, 1957.

is he there for ? " . . . " Just for *fun*, Thomson," replied Maxwell. When we consider a simple incongruity of this type, we can see why this form of humour is sometimes described as " nonsense " ; for " sense " implies the normal order of things, and in this order an incongruity makes " nonsense." A simple incongruity in the literature of physics is R. W. Wood's recording of the fact that he cleaned out an optical instrument by pushing his cat through it.

Incongruities are always lurking round the corner, to be called forth even by an errant comma. I saw a report of an address by a Low Church minister in which, speaking of vestments, he had said : " I want to wear no clothes which would distinguish me from any other man." The printer, however, had inserted a comma after " clothes," with impressive effect.

Even a change of dimension is sufficient to cause an incongruity. Lord Cherwell has a story of a scientist at Farnborough in World War I, who was so dismayed by the delays in ordering commercial equipment that when he wanted a dark-room lamp he made a pencil sketch of one, to be made up by the workshop. It availed him little, however, because a proper engineer's drawing had by regulation to be made in triplicate before the workshop would start. Weeks elapsed, and finally after a knock on his door two workmen wheeled in the largest dark-room lamp ever constructed. In making the workshop drawing the draughtsman had left out one dash, with the result that intended inches became actual feet. One of the classic incongruities of this type is that due to Benjamin Franklin in a letter to the Editor of a London newspaper in 1765, chaffing the English on their ignorance of America : " The grand leap of the Whale up the Falls of Niagara is esteemed, by all who have seen it, as one of the finest spectacles in Nature ! " It is not, however, necessary for humour that the change of dimension should result in exaggeration. Understatement is at least as effective. We shall long remember Sir Winston Churchill's comment on General Weygand's 1940 foreboding that in three weeks England would have her neck wrung like a chicken : " Some chicken, some neck ! "

A variation on the simple incongruity in humour is to produce a congruity where incongruity is normally expected. One does not expect, for example, any congruity about the names of joint authors of scientific papers. It was therefore rather a surprise to find a genuine paper by Alpher, Bethe and Gamow, dated April 1 in *The Physical Review* for 1948.* And one would be equally surprised to find a reference to ecclesiastical doctrine in a modern scientific paper ; but Gamow contrived to make a relevant reference to an address by Pope Pius XII in a *Physical Review* paper in 1952. Many puns fall in this unexpected congruence class, and are specially notable in naval signals. The most recent volume of the naval history of the 1939-45 war reminds us of the commander of one of our submarines who signalled to an accompanying destroyer at the beginning of an action " I intend to remain on the surface as long as possible " and received from the destroyer commander the reply " And so do I ! "

The capacity to perceive congruence among apparent incongruities is one of the essentials of wit, and as an example we may take the incident at a peace conference after Waterloo when the French Marshals ostentatiously turned their backs on the Duke of Wellington as he entered the room. " I have seen their backs before," observed the Duke. Indeed, this capacity for perceiving congruences is akin to that of the search of science for underlying unity.

A further variation of humour is produced when a false incongruity is expected by the victim, and an incongruity then genuinely occurs which he promptly discounts. The late Sir Francis Simon had this happen to him when he was head of a laboratory in Germany. One night his research students were working with liquid hydrogen, and there was an explosion which damaged the laboratory some time after midnight. One of the research students telephoned the

* Dr. Gamow says in his *Creation of the Universe* that there was afterwards a rumour that " when the α, β, γ theory went temporarily on the rocks, Dr. Bethe seriously considered changing his name to Zacharias," and that a fourth collaborator stubbornly refused to change his name to Delter.

professor to inform him of the damage. All he could get from Sir Francis was an amiable " All right, I know what day it is ! " It was the morning of April 1.

HOAXES

Simple incongruities, direct or inverted, can be humorous enough, but the more advanced jokes usually involve a period of preparation and induction, sometimes elaborate, before the incongruity becomes apparent. They are then called hoaxes. Maxwell's jokes were often simple in their preparation ; he is credited with having engineered the advertisement of his Inaugural Lecture at Cambridge (which is still very well worth reading) in such a manner that only his undergraduate students heard of it, and he gave it to them alone. The senior members of the University merely saw that the new professor would deliver his first lecture on a particular day, and they attended in force. This lecture, however, was the first of his undergraduate course, and his delighted students enjoyed the experience of seeing Maxwell gravely expounding, though with a betraying twinkle in his eye, the difference between the Fahrenheit and Centigrade scales to men like Adams, Cayley, and Stokes.

With some hoaxes the period of induction of the victim may be extended. In this type, which is probably the most interesting philosophically, the object is to build up in the victim's mind a false world-picture which is temporarily consistent by any tests that he can apply to it, so that he ultimately takes action on it with confidence. The falseness of the picture is then starkly revealed by the incongruity which his action precipitates. It has not proved difficult, for example, to persuade a Doctor of Philosophy to lower his telephone carefully into a bucket of water in the belief that he was co-operating with the engineer in the telephone exchange in finding a leak to earth. The prior induction consisted of building up in his mind a picture of something being wrong with his telephone by such tactics as repeatedly ringing the bell and then ringing off as he answered.

As a further example, we may recall one of the works of a German physicist, Dr. Carl Bosch, who about 1934 was working as a research student in a laboratory which overlooked a block of flats. His studies revealed that one of the flats was occupied by a newspaper correspondent, and so he telephoned this victim, pretending to be his own professor. The " professor " announced that he had just perfected a television device which could be attached to a telephone, and which would enable the user to see the speaker at the other end. The newspaper man was incredulous, but the " professor " offered to give a demonstration ; all the pressman had to do was to strike some attitude, and the voice on the telephone would tell him what he was doing. The telephone was, of course, in direct view of the laboratory, and so all the antics of the pressman were faithfully described. The result was an effusive article in the next day's paper and, subsequently, a bewildered conversation between the true professor and the pressman.

The induction of the victim can take many forms. One of the favourite ways is an acclimatization by slow change. R W. Wood is said to have spent some time in a flat in Paris where he discovered that the lady in the flat below kept a tortoise in a window pen. Wood fashioned a collecting device from a broom-handle, and bought a supply of tortoises of dispersed sizes. While the lady was out shopping, Wood replaced her tortoise by one slightly larger. He repeated this operation each day until the growth of the tortoise became so obvious to its owner that she consulted Wood who, having first played a subsidiary joke by sending her to consult a Professor at the Sorbonne whom he considered to be devoid of humour, advised her to write to the press. When the tortoise had grown to such a size that several pressmen were taking a daily interest, Wood then reversed the process, and in a week or so the tortoise mysteriously contracted to its original dimensions.

Induced incongruities mutual to two victims are possible. Each victim is led to believe in a false world-picture which is nevertheless consistent with and complementary to the false world-picture of the other victim. A simple example is the device of privately telling each of two people whom one

is about to introduce that the other is a good fellow who has, however, been going through a severe nervous strain, and who is therefore apt to get both irritable and rude if contradicted. It is therefore advisable to humour him by agreeing with all that he says. The two victims then go to great lengths to agree with one another, and separate in the conviction that each has handled the other extremely well.

HOAXES IN WAR

Induced incongruities have a high place in warfare, where if the enemy can be induced to take incorrect action the war may be advantageously affected. A stratagem in which some of my wartime colleagues were involved is now well known as " The man who never was." These same colleagues also worked with me in some technical deceptions, of which one was the persuasion of the Germans in 1943 that our successes against the U-boats were due not to centimetric radar but to a fictitious infra-red detector. We gained some valuable months while the Germans invented a beautiful anti-infra-red paint and failed to find the true causes of their losses. The paint, incidentally, was a Christiansen filter of powdered glass in a transparent matrix over a black base. The filter " peaked " in the near infra-red, so that incident radiation in this region went through and was absorbed in the underlying black. Visible light was scattered back by the filter, which thus gave a light grey appearance to the eye, but was black to the near infra-red. This simulated admirably the reflecting power of water, and thus camouflaged the U-boat. It was afterwards reported that the inventor of the paint was Dr. Carl Bosch.

We must readily admit that not all the successful hoaxes in the war were British. The book *Operation Nordpol* tells of a hoax on the British sabotage organization by a German security officer, Colonel Giskes. Although he had captured several British agents, Giskes succeeded in making it appear that they were still at large, and persuaded us by counterfeit radio messages to keep them supplied with sabotage equipment. The peak of his achievement was when he persuaded us to drop them by parachute a supply of tennis balls instead of explosive, on the grounds that the agents were in touch with the King of the Belgians who was very fond of tennis, and that the balls would therefore make excellent douceurs in securing his aid. I hope that our admiration for such technique will always rise above the havoc of war. A sense of history ought to have warned us that tennis balls were a touchy international commodity ; an earlier consignment had led to Agincourt.

While on the international plane, I would tell you of another hoax in Intelligence, this time by a very gallant Frenchman whom I knew during the war. He was one of our agents, and on one of many occasions he parachuted into France with about half a dozen radio transmitters. He managed to hide them at his base, a farm, but he was faced with the problem of distributing them to his sub-agents. This was a dangerous operation, but he executed it in the following manner. He went into the nearby town, and made friends with the local German commander, to such effect that he was able to put to the German the proposition that he was in the black market and stood to make a considerable sum by selling a load of carrots. The only thing that was holding up the deal was his ability to deliver the carrots to the buyer ; he therefore offered the German half the profits if he would make available a German Army lorry with a driver. The German agreed, and the lorry called at the farm where the carrots lay—and where the radio sets were hidden. A large paper-covered package was carried on to the lorry, and covered by carrots. Naturally, the driver was suspicious ; our French friend, however, tore open one corner of the package before it was finally covered and drew out a packet of a hundred cigarettes. These he thrust into the driver's hands, hinting that the black-market carrots were a mere cover for even more black-market cigarettes, and promising the driver another hundred when everything was delivered. The driver was now content, both with the cigarettes and with the knowledge of the hoax that was being played on his superior, and duly delivered both

carrots and package to the customer. The hero—and well he deserves the title—then went into the town a few days later and paid the German officer with money which had also been parachuted into France.

There still remained the problem of further delivery of the sets. If n had originally been dropped into France, one had been left at the first resting point, and $(n-1)$ were with the carrots. The hero therefore pointed out to the German commander that the whole transaction had been black-market, and that if only the commander could somehow discover it he was entitled to confiscate the carrots. What was more, our friend had a new customer, and could therefore repeat the whole operation to their mutual profit. The whole performance therefore took place again, the only difference being that there were $(n-2)$ sets concealed in the carrots this time. I do not know how many more times it was repeated before the carrots went rotten.

Before I turn to the more serious side of this lecture there is one further story from Physics in which the exact classification of the incongruity can be left as a problem to be worked out at leisure. It concerns Lord Kelvin's lectures at Glasgow, where he used to fire a bullet at a ballistic pendulum ; as an undergraduate at Oxford I had heard a story of how Kelvin missed on one occasion, with the result that the bullet went through a wall and smashed the blackboard of the lecturer next door. Kelvin rushed into the next room in some alarm to find the lecturer unscathed, and the class shouting "Missed him —try again, Bill." This experiment has now produced a further incident, and to avoid any doubt I wrote to Professor Dee for his own account of what happened. This is what he says :

" In the Quincentenary Celebrations here I had to lecture on the history of the Department. Of course Kelvin figured strongly in this. One of Kelvin's traditional experiments was to fire a rifle bullet at a very large ballistic pendulum. All his students regarded this as the highlight of the course. He was reputed to have the gun charged with a big dose of powder—the barrel is about half an inch internal diameter. I decided this experiment must be repeated but there was great alarm here that the barrel would burst and annihilate the front row (Principal and Senate). So I decided to use a modern rifle. I also decided to make it a double purpose experiment by using Kelvin's invention of the optical lever to display the pendulum swing to a large audience. On the night all went off well.

The next day I repeated the whole lecture to the ordinary class. Mr. Atkinson was the normal lecturer to this class and he had noticed that in referring to the dual purpose of the demonstration I used the phrase " . . . fitted a mirror to the pendulum so that I may kill two birds with one stone." After the explosion to my surprise a pigeon fell with a bloody splash on to a large white paper on the bench—our lecture room is very high. I tried to resolve the situation by saying " Well although Mr. Atkinson isn't lecturing to you to-day he appears to be behind the scenes somewhere. But he does seem to have failed to notice that I said *two* birds with one stone ! " Immediately a second pigeon splashed on the bench ! Whether this was due to a slip up in Atkinson's mechanical arrangements or to his brilliant anticipation of how I would react I don't really know but I always give him the credit of the second explanation.

Anyway the students loved it but I wonder how many would remember about the optical lever ? "

When the foregoing example has been evaluated, the following one may be considered. It was a well-known story a few years ago when we were inclined to laugh at Russian science. It concerns a Russian professor who trained a cricket to jump when commanded. He demonstrated this to his students and then said : " Gentlemen, we will now remove the cricket's hind legs." He did so, and again commanded the insect to jump. After repeated failure he said " Gentlemen, from this experiment we can surely conclude that when we remove a cricket's hind legs, we deprive it of its sense of hearing." At this point, the story normally ends, because we physicists think that the professor is a victim of a self-inflicted incon-

gruity. However, a cricket really does have ears in its legs, and our laughter at the professor's folly is only just saved by the fact that they are in its fore legs. There is an obvious moral to be drawn.

TECHNICAL SPOOF IN WAR

I want to turn now to technical deception in war, as exemplified by our attempts to mislead the German night defences in their appreciation of our raiding intentions. The method here is that of the induced incongruity ; by a false presentation of evidence we wish the enemy controller to build up an incorrect but self-consistent world-picture, thus causing him to generate the incongruity of directing his nightfighters to some place where our bombers are not. I originally developed this " Theory of Spoof " in a wartime report ; the salient points, which have some interest in physical theory, are the following. As with all hoaxes the first thing is to put oneself in the victim's place (indeed, a good hoax requires a sympathetic nature), to see what evidence he has with which to construct and test his world-picture. In night aerial warfare in 1939-45, this evidence was mainly the presence of deflections in the trace of the cathode ray observing tube. Therefore any device which would give rise to such deflections could provide an element of Spoof. One such device was a jammer which would cause fluctuating deflections all the time, thus concealing the true deflection due to the echo from an aircraft. This, like a smoke screen, would render the enemy unaware that you are where you are. A more positive technique is to provide a false echo, and if possible to suppress the genuine one, thus giving him the impression that you are where you are not. The easiest way of providing a false echo is to drop packets of thin metal strips, cut to resonate to the enemy's radar transmissions. This is, of course, what we did in 1943. There is little time to tell now of the fortunes of this technique, but the packets were extremely successful, and they changed the character of air warfare at night. At first, the German controllers confused the individual packets with aircraft ; I can still remember the frustrated tones of one controller repeatedly ordering a packet to waggle its wings as a means of identification. Soon, however, the Germans gave up the attempt to make detailed interceptions, and tried to get a swarm of fighters into our bomber streams. We then used many tinfoil packets dropped by a few aircraft to provide the appearance of spoof raids, which lured the nightfighters off the track of our main raids.

At this stage we witnessed a few self-inflicted incongruities, where the German controllers generated their own delusions. I myself heard this happen with a Bomber Command raid on Bochum. Another example is recorded in General Galland's book *The First and the Last*. In those days when German radar was blotted out by the metal strips (" The fateful hour of the Luftwaffe "—Galland), Goering used sometimes to take personal control of the defences. With one American raid, above the clouds by daylight, Goering decided from the line of flight that the target would be the ball-bearing works at Schweinfurt. He therefore ordered his fighters to fly back towards this town. His ground observers then reported the noise of aircraft flying towards Schweinfurt ; actually these were his own fighters, which could not be identified above cloud, and Goering took the evidence as confirming his guess about the target. From then on the mistake became self-propagating. No bombs were dropped on Schweinfurt, and as some of the fighters overshot Goering looked for the next target further East. This was the Leuna works in Leipzig, and so he ordered his fighters there. Again the ground observers duly reported the stream of aircraft, and again there were no bombs on Leipzig. So Goering had another guess, this time the Skoda works at Pilsen in Czechoslovakia. On went the fighters at his command, until the cloud cover slowly broke up to show not an American aircraft in the sky. The American target had been Düren in the Rhineland, more than 300 miles away, and the Americans were safely on their way home long before the Reichsmarshall was disillusioned. It says much for Goering that he thereupon sent a signal to all units in which he congratulated himself and all participants

on the " successful defeat of the air raid on the fortress of Köpenick "—the latter being a reference to one of the greatest hoaxes yet perpetrated, where before the first World War a private citizen disguised as an Army officer paralysed all the civil machinery of the garrison town of Köpenick.

As the war went on the Germans gradually found ways of distinguishing between echoes from metal foil packets and those from aircraft. The packets, for example, resonated to one particular frequency, and therefore they had a relatively poor response to another frequency. If two radar stations watched on widely separate frequencies, a genuine aircraft echo would be present on both, whereas the foil echo would appear only on one. The foil could, of course, be cut to different lengths, but as the number of frequencies was increased, the amount of foil needed was greater. Moreover there was a pronounced Doppler effect on the echo from an aircraft, with its high speed, but little effect on the echoes from the foil drifting with the wind. Thus, against an omniscient controller, we have to make the decoy echoes move with the speed of aircraft, and reflect different frequencies in the same way. This is easiest done by making a glider of the same size as the bomber. Then if we allow the enemy controller to use sound and infra-red detectors and other aids, we find that the only decoy which can mislead him into thinking that there is a British bomber flying through his defences is another British bomber flying through his defences.

Another example is one that I encountered earlier in what has been called " The Battle of the Beams " in 1940. Here the problem was to upset the navigation of the German night bombers, when they were flying along radio beams to their targets. The signals received by the pilots telling them to steer right or left were counterfeited in this country, and sometimes resulted in their flying on curvilinear courses. However, had the pilots had unlimited time of observation they could have detected that there was something wrong, even if we had exactly synchronized our transmitters with those of the Germans. The bombers were in general flying away from their own transmitters and towards ours, and so they would have received a Doppler beat from which they could have deduced that a second transmitter was active. If one allows the possibility of various fairly simple tests, which fortunately would take too long in actual warfare, one arrives at the conclusion that the only place for a second transmitter which will simulate the original exactly is coincident with the original and the counterfeit thus defeats its purpose.

In war, of course, neither controllers nor pilots are omniscient, and they have limited time and means of observation. They can therefore be misled ; but if we grant the possibility of unlimited observation, then we can see from these simple examples that no model can be perfect unless it is an exact replica of the original—and even then the perfection is spoilt by the fact that now two exist where there was one before.

The ease of detecting counterfeits is much greater when different channels of examination are used simultaneously. This is why telephonic hoaxes are so easy—there is no accompanying visual appearance to be counterfeited. Metal strips were most successful when only radar, and that of one frequency, was employed. Conversely, the most successful naval mines were those which would only detonate when several different kinds of signal, magnetic, hydrodynamic and acoustic, were received simultaneously. A decoy which simulates all these signals is getting very like a ship. From these considerations, incidentally, we can draw a rather important conclusion about the detection of targets in defence and attack : that as many different physical means of detection as possible should be used in parallel. It may therefore be better in some circumstances to develop two or three independent means of detection, instead of putting the same total effort into the development of one alone.

THE SUCCESSES AND LIMITATIONS OF PHYSICAL MODELS

Now the thesis that no model can be perfect is at the root of physical theory. In fact, when we realize that this is so, we ought

to be rather impressed that models and analogies in physical theory have been so successful. When we say that we have "explained" some new physical fact, what we usually mean is that we have been able to interpret it in the light of our previous experience, i.e. to form an analogy between it and something we already know well. We "explain" the properties of gases on Kinetic Theory, by forming an analogy between gas molecules and ball bearings; and from the mechanical properties of ball bearings, which are a matter of everyday experience, we form a theory which predicts reasonably well the properties of gases. We began to feel we knew something about the atom when Rutherford formed his analogy between the electron structure and the solar system which we already knew, and when Niels Bohr formed his analogy between the stability of the nucleus and that of a charged liquid drop. More recently, the shell structure of the nucleus has had some analogy with the outer electronic structure, and Yukawa's prediction of the meson was from analogy between nuclear force fields and electromagnetism. There are many other examples which spring to mind, and the mechanistic view of the late nineteenth century was largely due to the misleadingly great success of analogy with mechanical models. The limitations of such models ought, however, to be realized, and indeed anyone who has tried practical model-making will realize that even a change of scale is enough to make a model break down. There is a limit, for example, which is reached in the making of model railway engines set by the atomic nature of matter. And anyone who tends to cling too enthusiastically to any one model in physics should heed the ninety-seventh Psalm: "Confounded are they that serve graven images." It is, of course, true that in physical observations we do not have unlimited time of observation, and it is therefore possible that a model may simulate completely all the properties of the original that we have been able to observe. Differences between the model and the original will, however, appear on further observation.

In physical thought we have in the main progressed by the process of inventing a model using components which are already well established in our experience to simulate the behaviour of entities in new fields of experience. In this connection it is interesting to note the theory of William James that ability to reason in new situations by analogy with previous experience is the true criterion of intelligence. Having regard to the limitations of models brought out by the preceding discussion, however, we must not be surprised when even the most successful models ultimately break down; by a breakdown we mean that some aspect of behaviour of the model leads us to a false prediction of a property of the entity. Thus the classical "paradox" about the wave and particle theories of radiation is no paradox at all. There are some properties of radiation which are best simulated by the model of a stream of corpuscles, miniaturized from a macroscopic concept of things the size of ball bearings. There are other properties which are best simulated by a model using wave motion miniaturized from concepts of mechanical waves at sea or on strings. Ought we to be surprised when either model breaks down? The truth is that light is not, and cannot in principle be, the same as either limited model, and we ought not to be shocked when we find that it can be simulated in part by each of two (and there might even be more) imperfect models. Each model is nevertheless extremely helpful in predicting some of the actual properties of light, and danger arises only when we forget its inherent limitations.

I have mentioned the breakdown of models caused by a change of scale, and we have, of course, had much experience in this century of the difficulties of making models of elementary particles using macroscopic concepts. The difficulty may become clearer if we imagine a thinking being whose everyday experience is with bodies of dimensions of the order of a million miles. Now, owing to the preponderance of gravitational forces, which would cause plastic flow inside large solid bodies, most of the regular bodies that he sees will be approximately spherical, or at most flat discs. It might be extremely

difficult for him to visualize a body of cubical shape. Now if he discovered elementary elastic particles the size and shape of bricks, he might be hard put to it to " explain " the properties of bricks, such as the preferential ways in which they could be assembled together, or their rather peculiar collision properties, because there would be no analogy with bodies in his everyday experience.

It is fascinating to speculate conversely about phenomena involving large dimensions, because we may in fact have difficulty in visualizing the paramount forces. Just as on an atomic scale gravity is a negligible force, it is possible that on the galactic scale there is a force which is negligible in any laboratory experiment that we can perform. Such a force, for example, might be a small repulsion diminishing at some rate less than the inverse square. This would make it of comparable importance to gravity at great distances, and might help to explain some of the structural features of the galaxies and the universe. And it would be in general keeping with the present context if we revive Crabtree's eighteenth century concept of the Force of Levity.

We have seen in the last hundred years the rise of the mathematical model, where mathematical symbols and operations become the components. This is especially true of Heisenberg's matrix mechanics, and such models have been very profitable. With Maxwell we must admit that :

" There are, as I have said, some minds which can go on contemplating with satisfaction pure quantities presented to the eye by symbols, and to the mind in a form which none but mathematicians can conceive. There are others who feel more enjoyment in following geometrical forms, which they draw on paper, or build up in the empty space before them. Others, again, are not content unless they can project their whole physical energies into the scene which they conjure up. They learn at what a rate the planets rush through space, and they experience a delightful feeling of exhilaration. They calculate the forces with which the heavenly bodies pull at one another, and they feel their own muscles straining with the effort. To such men momentum, energy, mass are not mere abstract expressions of the results of scientific inquiry. They are words of power, which stir their souls like the memories of childhood.

For the sake of persons of these different types, scientific truth should be presented in different forms, and should be regarded as equally scientific, whether it appears in the robust form and the vivid colouring of a physical illustration, or in the tenuity and paleness of a symbolical expression."

We can therefore see physics proceeding by both mathematical and physical models. The new frontiers of to-day must be converted into the everyday experience of tomorrow, so that the younger physicists feel entirely at home in it, and can draw upon its phenomena to form in turn the basis of models for " explaining " the new phenomena which they will encounter. We might be tempted to ask whether there is any end to this process, but I for one am unable to answer. It will be time enough to think about that when we encounter a long period without advances.

With Bacon, I see the physicist in much the same position as an intelligence officer, trying to assemble and interpret the evidence of Nature. Because of his limited means of observation, he will often build up a false world-picture from biased clues ; but just as the intelligence officer can check his mental picture of the situation by making further observations, so can the physicist correct his model by further experiment. We often hoax and mislead ourselves when we attempt to understand the natural world, but we can usually recover the right track by enough effort in both theory and experiment. We should not be discouraged by the inevitable failure of models ; instead we should be grateful for their tremendous fruitfulness.

The m.k.s. System

(TWO REPLIES TO PROFESSOR H. LIPSON)*

(1) By G. R. NOAKES, M.A., F.Inst.P.

(*Uppingham School*)

THE case for using the rationalized m.k.s. system of units in the teaching of electricity has, it must be admitted, been presented from time to time with misjudged vehemence and enthusiasm. Its advantages to the electrical engineer have also been urged with a salesmanship which would not necessarily commend it to the teacher of physics. That, on cool and considered examination, it has other genuine advantages which will enable a better presentation of elementary physics to be given is nevertheless the opinion of some very experienced teachers at Professor Lipson's own level[1, 2] The Report [3] which he associates with such words as " passionate " and " pompous " should not be dismissed merely because it was compiled by physicists who are (or were) primarily schoolmasters. I happened to be a member of this sub-committee, and know the frame of mind in which the Report was approached and concluded. Many of us came to the task with misgivings and doubts, which were replaced by utter humility when we realized how much we had still to learn about the very first principles ; but we ended honestly believing that we understood what we were doing. I should add that if Professor Lipson were in fact quoting wholly from this Report in his presentation of the m.k.s. system, then we should have much to answer for ; but he is not. The statements he makes about the system do not represent it fairly. I feel that he is not really clear about the basis of *any* system of units, and that this is the real trouble. There is no short answer to his criticisms except to suggest that he reads the Report through again, this time in conjunction with McGreevey's and Cotton's books which indicate how the system can be embodied into a teaching course. But when in effect he says he is no expert—as Brutus is —but, as you know him all, a plain blunt physicist, the implication must be taken up. It is that of a panel of ushers, with their ferules swollen into metre-sticks, setting themselves up as experts. This is not so much unkind as absurd. No expertise can attach to things like units, dimensions, graph-paper, or slide-rules. But the four things have common features—some sorts are better than others for a particular problem, and you have to understand enough about the problem itself to make the happiest choice of tools.

May I take his article point by point ?

First, the experimental aspect. The chief advantage claimed for the new treatment is its more direct experimental basis. The Goodier current-balance and the fluxmeter experiments of the late F. A. Meier offer something much more acceptable to pupils than current-elements and the tangent galvanometer. Next, although the phrase may possibly have been used with reference to teaching method, nobody has ever suggested that magnetism is " unnecessary." The basic experimental fact of magnetism can be interpreted as the interaction between dipole and field ; unnecessary *units* are avoided if quantitative measurements start by considering the field, rather than one pole of the dipole, first ; so is much algebra which is both unfruitful in itself, and a poor substitute for the proper mathematical treatment which would at a later stage presumably have some instructive value.

His point about the definition of the ampere goes a little deeper. I am not quite certain

*Lipson, H. *The m.k.s. system*, *Bulletin*, January, 1957, p.13.

what he means by an absolute definition, as distinct from an absolute unit, or why a unit must produce unit effect in unit circumstances. How about the joule as a heat unit, the candela as a flux unit, and the mole as a mass unit ? From this point of view the figure 2×10^{-7} is a conversion factor of the same essential nature as 4·2, 4π, or the appropriate molecular weight. But I must take up the question of arbitrary choice and absolute measurement. First, let us suppose that the ampere, however it be defined or realized practically, is arbitrarily chosen as the fourth fundamental unit, to rank beside the metre, kilogram, and second, It need not conform, in principle, to Professor Lipson's criterion or to any other. It happens to be what it is made. Nothing whatever would suffer if it were still defined in terms of the odd milligram of silver per second. The 2×10^{-7}, or the 0·001118 (times 10^{-3} for kg.), do not sacrifice the independence of the fourth unit, but relate it to the other three for convenience in reproducing it. If this is agreed, then *no other arbitrary choice is possible at all ;* for the values of μ_0 and k_0 are both settled by direct experiment. The unit system would then be properly known as the m.k.s.a. system.

However, the ampere need not have been chosen. It was so chosen because it happened already to be there as the practical unit. Its status, but not its size, has been changed. A force of one newton per metre length would be given by currents of about $2{\cdot}3 \times 10^3$ amperes in the conductors of the definition. It would be logical, in terms of unitary method, to take such a current as the unit ; it is not logical in terms of the scale on which electromagnetic actions happen to occur ; and as the whole object of the exercise is ultimately thrift, how fortunate we are to be able to use up the old ampere by accepting the factor 2×10^{-7} !

But, it is said, the value of μ_0 *has* been arbitrarily chosen ! Let us be clear ; just because a value of something is arbitrarily chosen on one c.g.s. system, it does not mean that its value in some other system is also chosen—it may be a measured or derived value. Thus, on the absolute c.g.s. e.m.u. system, μ_0 is 1 e.m.u. and k_0 is $1/c^2$ e.m.u. ; the former is chosen, the latter results from the choice. Similarly, on the absolute c.g.s. e.s.u. system, k_0 is 1 e.s.u. and μ_0 is $1/c^2$ e.s.u. —one arbitrarily chosen, the other derived. (What is c^2 ? A *measured* value, of such interesting size and dimensions that Maxwell looked into the matter further.) On the m.k.s.a. system I is chosen and both μ_0 and k_0 follow therefrom. However, as it is alleged that μ_0 is arbitrarily chosen, and this may conceivably be right, let us examine the conditions in which such choice could be made. Suppose the sizes of both μ_0 and the current unit to be as yet undefined, and a current balance set up to measure the force or torque between two coils. With this, we can measure a quantity recognized as $\mu_0 I^2$ in terms of M, L, and T. But neither μ_0 nor I can be measured independently of the other in terms of the three existing units. One of the two must be selected, to make a fourth fundamental unit. If the ampere is defined as the fourth quantity the value of μ_0 is measured by such an experiment.

If the value of μ_0 is arbitrarily chosen, as the fourth quantity, to be $4\pi \times 10^{-7}$, then the ampere follows as a *derived* unit. The system would then be called the " m.k.s.μ_0 " system. The element of arbitrariness is to some extent controlled, for μ_0 must really have such a value as to make the current unit the same size as the old practical unit ; but it exists nevertheless. And for all that absolute current-balance measurements can tell us, this line of argument might with an ill-considered choice for μ_0 have led us to an absurdly sized unit of current. In fact, as we insist on using the ampere, we take no risks and nail down the unit of I by definition; this is a decision. Whether the result is thought of as m.k.s.a., or m.k.s.μ_0 doesn't really matter much, although strictly the definition to make sure of the ampere has taken the former line. What is important is that you mustn't mix the two up. You cannot, in one and the same system, have both (*a*) the ampere as an arbitrarily chosen absolute unit, and (*b*) the value of μ_0 as arbitrarily defined. You must take one or the other, but only one.

Taking matters one stage further, having settled preferably for (*a*) but possibly (for the sake of argument) for (*b*), consider the status of k_0. This cannot in either case be arbitrarily chosen. Indeed, it could never be arbitrarily chosen except as the basis of an electrostatically-based "k_0 system." Once the volt has been established using the energy relation, k_0 is measurable directly by finding k_0V^2 in mechanical units with an electrostatic voltmeter, or by measurement of a calculated capacitance. What Professor Lipson says really implies that he believes that a system of electrostatic units has somehow been slipped back into the m.k.s.a. system, and that the article offered as sound is in fact two fragments loosely secured together by guile. I feel that this is the ultimate source of the difficulty ; and that it may be based on the emotional attachment to $k_0 = 1$ and $\mu_0 = 1$, without overt recognition that this happens in two separate systems.

Next, the statement of the current-element formula $dH = Id\delta s \sin/4\pi r^2$. Basically, this is needed not so much for H calculations as for B, finding the forces between circuits. As it is the differential form of the First Circuital Theorem, it would seem equally sound in principle to use the latter in its place for initial steps in field calculations. There is, I know, some division of opinion about this, and it would not be right to pretend that there is a single good method to which all others are inferior.

The quantities μ_0 and k_0 are to be found respectably enshrined in Maxwell's "Treatise,"(4) to delve no further into authorities. They are not "arbitrary constants," but *named* as properties of "the medium," for Maxwell would have preferred space filled with an ether. (The *numerical value* of one quantity was arbitrarily chosen in each system.) Whether or not we regard μ_0 as arbitrarily chosen now, they are certainly not entirely new concepts, except that we speak nowadays of "space" rather than "ether," though the latter may perhaps be rehabilitated one day. The only novelty is in having them side by side in one system of units, instead of having each as unity (arbitrarily !) in its own system and introducing the c^2 factor when both occur in one calculation together. How would Professor Lipson expound, say, Thomson's e/m experiment without doing this, and so raising the curtain prematurely on the drama of $\mu_0 k_0 = 1/c^2$? It is perhaps irrelevant (but the matter was mentioned) to discuss the sequence in which Maxwell himself introduced the electromagnetic theory of light. No dramatic disclosures suddenly unfolded, but a gradual infiltration of the idea in the course of a chapter on units ! This may not fit in with a teacher's own ideas nowadays, but there is nothing awkward or improper surely in doing things in Maxwell's own order ?

While on this point, it appears that Professor Lipson takes the speed of light in space to be a property of light rather than a property of space, which, he says, has no physical properties. Here one hastily changes the subject from Maxwell's theory ; for the proposition that both k_0 and μ_0 are not properties of space, followed by the same implied statement about c, just puzzles one, though it is at least consistent. What is to be said about the intrinsic resistance of space, which is not *defined* but *measured* as 377 ohms ?

The "field theory in which the force between two currents is accepted as basic," or its equivalent, does in fact exist. The force between current-carrying conductors can be calculated by applying Coulomb's law, and the appropriate relativity correction for moving charges. I can remember seeing it in print twice within the last ten years and am sure enough of the authors to be able to verify the references quickly when petrol permits.(5, 6) In principle, the idea is not new. Maxwell gives formulae derived by Weber and by Gauss for this result ; but, since the special theory of relativity was needed for the complete solution, these attempts were only partially successful.

In conclusion, I do not think that Professor Lipson has given a biased view of the new system ; I should call it over-simplified. I cannot tell whether the foregoing remarks will help him, or anyone else ; for the more

one tries to clarify matters, the more elaborate one makes them seem.

The *soundness* of the system is explained fully in the I.E.E. Symposium,(7) and more concisely in a familiar book which will appeal perhaps more authoritatively to the experimental physicist.(8) As to teaching, if I might offer two suggestions, the first is to arrange the course so that the philosophy of units cannot come into things too early ; whether it is ethical or not, the students must take some matters on trust at first. The second is to teach *about* units in general, as one does about dimensions, at a suitable stage in the course before applying any one system to deduce formulae. An immense amount of work can, in fact, be covered on the basis of the old "practical units"—that is, postponing *discussion* of the new ampere definition—and much that is traditionally sweated out the hard way given elegantly by the use of the law of conservation of energy, before this stage is reached. For example, unethical though it may be, I would teach the whole of alternating-current at Advanced-level practically, measuring L and C and X and Z and so on, *before* giving the theory of the simple alternator and deriving formulae deductively. The theory has to be done soundly of course—but when the time is ripe for its understanding. The same considerations apply to the theory of " measurement and units."

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Professor Lipson comments :—

I am pleased that Mr. Noakes has thought it worth while to reply to my article on the m.k.s. system. I think that it is necessary that members of the Institute should see both sides of the quesion, and I hope that others will also join in ; in this way, we may well find that there are improved methods of teaching, making the best of both worlds. I do think, however, that he himself exposes some weaknesses ; his admission that " the more one tries to clarify matters, the more elaborate one makes them seem," appears to me to show that the m.k.s. system *is* elaborate, whatever its supporters claim.

I do not propose to try to answer all the points that Mr. Noakes raises, because he has, to some extent, missed the point of my thesis ; I was concerned solely with the m.k.s. system as used for *elementary* teaching, and he goes far beyond this. How many physicists, for example, are acquainted with the concept of the " intrinsic resistance of space ? " My main thesis is that standard physical methods are accepted for other branches of physics, and I think that it is confusing to the student to introduce electricity and magnetism by methods that are essentially different.

Take units, for example. Units were hammered into me ! The dyne is that force which moves unit mass with unit acceleration ; the erg is the work done when unit force moves through unit distance. It is simple to carry over these concepts into other branches of physics, and if the units turn out to be of inconvenient size, we can adopt more practical ones ; but we should not call them absolute.

Then again, it is satisfying to teach the various branches of physics as they were studied classically, and then to show how they have been unified. Heat and sound are branches of mechanics, but nobody suggests treating them, *ab initio*, in this way. What is more natural, then, than to accept magnetism as a property shown by magnets, and only later to show its connection with electricity ? I admit we derive some awkward

units, but I do believe that in this way a student derives more of the spirit of physics than he would by a system already unified in advance. (The S.M.A. book *does* dismiss magnetism rather summarily ; most of the familiar items are classed on p. 18 under " Topics which may be omitted.")

Finally, I must point out that I used the words " pompous " and " passionate " deliberately. The former I used because I think that it is the right word to describe the concept " the permeability of free space " ; after all we do not call the gravitational constant the " gravability of free space." The word passionate was not intended to be interpreted adversely ; I sometimes think that we could do with more passion in our affairs. I think it is not an unfair adjective to apply to the quoted phrase: " This country cannot afford the luxury of the point-pole approach " (p. 6).

I have received several private comments, almost all favourable and some quite constructive. One correspondent, however, is indignant that I should have assumed that the S.M.A. book represents the opinions of more than a small minority of members.

I hope that others will join in this discussion ; it should not be left only to Brutus and Mark Antony.

(2) By J. ALAN CHALMERS, M.A., Ph.D., F.Inst.P.

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INTRODUCTION

In his recent article in this *Bulletin*, Professor Lipson discusses the teaching of elementary electricity and magnetism using the m.k.s. system. I, too, have taught these subjects at University level to a class one year in advance of that taught by Lipson, and after an experience of four sessions, my views are almost completely opposite to those of Lipson.

We have no applied science in Durham, so that I introduced the m.k.s. system, not after pressure from applied scientists but because I believe it to be of positive advantage to physicists, as I hope to show in what follows.

The class with which I am concerned consists of first year Honours Physics students, first year Honours students in Chemistry and Mathematics and second year General students. Since some of the best students in the class have already covered much of the subject matter for scholarship level in G.C.E. and some of the weaker students have found much difficulty with the traditional methods of teaching electrostatics and electromagnetism, for both of these types of student there are positive advantages in a new method of approach, quite apart from the advantages of the system itself, and many students have appreciated the stimulus they have received.

DISCUSSION OF LIPSON'S ARGUMENTS

There are certain of Lipson's arguments which I feel it desirable to answer, before discussing what I consider the positive merits of the m.k.s. system.

In the first place, the definition of the ampere in terms of forces between currents is, I believe, only of historical interest. The International Ampere is defined in terms of deposition of silver and all instruments are calibrated, directly or indirectly, in terms of this definition. In dealing with the m.k.s. system we have to consider the ampere in just the same light as the metre, kilogram and second, as a standard which has been fixed for us precisely. Measurements yield the constant ε_0 and we shall find that it is quite close to the value corresponding to the historical definition of the ampère, just as a geographer will find the circumference of the earth to be close to the value corresponding to the historical definition of the metre, or a comparison of the standard metre and the standard kilogram would yield a value of the density of water close to, but not exactly, 1,000. For practical use, it is essential to have material standards, not theoretical units, or we should have to alter our calibrations every time anyone made a more accurate determination of fundamental relations ; for example, surely the

best theoretical unit of charge is that on the electron, but if the coulomb had been defined as the charge on a certain number of electrons, our instruments would have needed about a one per cent change since the time of Millikan's experiments ! I claim that it is a positive advantage of the m.k.s. system, or of the way in which I teach it, that the fourth fundamental unit is the practical ampère, not a theoretical conception.

Lipson is surprised that free space has the measurable properties of permittivity and permeability. But what about gravitation ? Surely G is a property of space.

The greater simplicity, for the elementary student, of a system in which B and H have the same value in air has, I believe, caused a great deal of confusion quite unnecessarily. It is as though a system of units had been devised to make g equal to unity, in order to avoid the schoolboy's difficulty in distinguishing between mass and weight ; so nice for the schoolboy, but how difficult to remove the misconceptions later on ! Incidentally, it seems to me a minor positive advantage of the m.k.s. system that it makes the density of water 1,000 and so removes the confusion between density and specific gravity. In passing, I would like heat to be measured in joules, not in calories, so that the specific heat of water is not unity, but is recognized as having a definite value and definite units.

If the geophysicist does not like to abandon his old units, he can retain them, as the British engineer has done ; in any case, ought he not to measure in oersteds rather than gauss ?

As regards 4π, it is a much greater shock, surely, to an elementary student, to find 4π lines of force issuing from a unit charge than it is to insert a 4π in a formula where it can be shown to arise from the 4π in the area of the spherical surface ; since the area of a sphere is $4\pi r^2$, it seems reasonable to find 4π associated with r^2 wherever there is an inverse square law. In the example Lipson mentions, of the magnetic intensity at the centre of a circular coil, the π arising from the inverse square law cancels the π from the length of the coil.

ELECTROSTATICS

Although the electrical engineer finds much advantage in the m.k.s. system in his calculations, it is equally of positive advantage in electrostatics.

I believe the approach to electrostatics from the inverse square law is needlessly difficult and the fundamentals of the subject are best dealt with by the alternative approach of the parallel plate condenser, as suggested by Nicholson[1]. Instead of considering imaginary experiments with point charges, we consider the uniform field in a parallel plate condenser and discuss continuous lines of flux with their ends on electric charges, one line per unit charge ; extending the idea of continuous lines of flux to non-uniform fields, the inverse square law and Gauss's theorem follow immediately, and the Cavendish-Maxwell experiment can be invoked to confirm the hypothesis of continuous lines of flux. " Flux " is here defined as field × permittivity, and since, even for a vacuum, the permittivity is not unity, there is no danger of confusing flux and field. Although this approach is new to students, it is, I have found, simpler to follow than the old, and, incidentally, does not require the rather troublesome idea, to the student who is not geometrically minded, of the solid angle in the " proof " of Gauss's theorem.

QUASISTATIC ELECTRICITY

By this title I refer to phenomena which lie on the border-line between static and current electricity, phenomena which usually are, but should not be, neglected in elementary courses. In such cases there are electrostatic fields and charges which are maintained in some way although there also exist electric currents moving the charges ; an instantaneous picture of the situation of the charges would be the same at different times, but the same charges do not remain in the same places. Examples of such phenomena occur in space charge effects in thermionics, in some gas discharge problems, in atmospheric electricity and in electrolytic boundary problems.

If one attempts to deal with such problems by the older methods, difficulties arise

in regard to units. In their old forms, Gauss's, Poisson's and Coulomb's theorems hold only in electrostatic units, but all measurements are made with instruments calibrated in practical units, so that it is necessary to convert from one set of units to the other, involving the right place to insert c and the right power of 10 to convert electromagnetic to practical units. The m.k.s. system obviates all these difficulties, since Gauss's and the other theorems are expressed in practical units and all one has to remember is one number, $\varepsilon_0 = 8{\cdot}854 \times 10^{-12}$. If any reader doubts the advantage of the m.k.s. system, let him calculate, by the old methods, the time taken for a current of 10^{-16} amperes/cm^2 to carry a charge equal to that which, on a plane surface, gives rise to a field of 1 volt/cm. In m.k.s., $\sigma = it = \varepsilon_0 F$.

So $t = \dfrac{\varepsilon_0 \times 10^2}{10^{-12}} = 885{\cdot}4$ seconds.

ELECTROMAGNETISM

My approach to electromagnetism on the m.k.s. system is to define the magnetic induction vector, B, by the force produced on a current element, starting at once with the difficult idea, which one cannot avoid, of the different directions of the induction, current and force. Next, one considers the relation between B and a current responsible for it, and from that the force between two currents, bringing in μ_0 as an empirical quantity since the unit of current is the standard ampere. Later, one can define the magnetic field strength, H, as B/μ and use this in Ampère's theorem. Finally, for the solution of some problems it is convenient to consider magnetic potential. In this treatment, magnetic poles are not brought in at all, and magnetic materials are considered only in so far as they affect the value of μ; this treatment can be considered as basing the measurement of current on the moving coil galvanometer rather than on the tangent galvanometer.

MAGNETISM

Magnetism has suffered long enough, surely, under the fiction of the unit isolated magnetic pole, which nobody ever hopes to find. Yet, in the old system the electromagnetic units must be derived from this fictitious entity. In the m.k.s. system, we deal with magnetic materials after, not before, electromagnetism and we avoid the isolated pole completely.

One of the most pernicious formulae in physics is : $B = H + 4\pi I$. This implies that B and H are quantities of the same nature and this misconception is enhanced by the textbook idea of cavities along and across the direction of the field. The equation is analogous to equating the quantity of heat to the rise of temperature, a result which is only true for unit mass of water.

Provided it is realized that B and H are essentially different, because μ is not merely a number, then the magnetic circuit is quite simple, and it is also possible to discuss magnetic phenomena in terms of internal, unmeasurable currents in the magnetic material as well as the magnetizing current. Lipson's compromise of using μ_0, but giving it a value of unity, is not likely to overcome the difficulties, since μ_0 will just be left out again !

UNITS AND DIMENSIONS

In most elementary courses on electricity and magnetism, a good deal of blood, sweat and tears is expended on the subject of units and dimensions. The teacher has two alternatives ; either he can use only the three quantities mass, length and time, and find that the same quantity has different dimensions on the two systems, a very unsatisfactory conclusion ; or else he can give dimensions to k and μ, although the previous work had implied that they were mere numbers, and he can reach a relation between the dimensions of k and μ ; but he then has fractional dimensions, difficult for the student to understand and forcing him to speak of " dimensions " not of units.

But in the m.k.s. system, there is, from the start, a fourth fundamental quantity, current or charge, and it is possible to get a consistent system to express all electrical quantities in terms of mass, length, time and current. One need not speak of dimensions at all but rather of units, obtaining units for all electrical quantities in

terms of metres, kilograms, seconds and amperes ; instead of using dimensional equations, the absence of fractional powers makes it possible merely to use the fact that the units must be the same on both sides of the equation.

Both ε (or k) and μ have units, and the theory shows that the units of $(\varepsilon\mu)^{-\frac{1}{2}}$ turn out to be metres/second, namely a velocity. The electromagnetic theory, which, of course, comes at a later stage in teaching, shows that this velocity is that of electromagnetic waves, but, unlike in the older methods, where this enters into the ratio of the units, there is no need to bring this velocity into elementary teaching.

CONCLUSION

In electrical engineering, the m.k.s. system has come to stay ; while we, in physics, are not bound to follow their example, I have tried to show that there are positive advantages to be gained from the adoption of the system. The advantages occur not only in the system itself, but also in the opportunities it gives for a changed approach to some branches of electricity and magnetism. Not only do students find the new approach stimulating but I have found, and so will others, that the change has led to a widening of interest and understanding for myself in the process of getting away from the traditional hide-bound methods of teaching.

REFERENCE

1. Nicholson, G. F. *An introduction to the rationalized m.k.s. system of units.* Brit. J. Appl. Phys., **2**, pp. 177-182 (1951).

Professor Lipson comments :—

Chalmers' reply to my article is the most helpful I have received ; unlike Noakes's reply, it keeps to an elementary level, and I do want to emphasize that I criticized the m.k.s. system solely in virtue of its claim to provide an introduction to magnetism and electricity for elementary students. I have enough respect for my electrical-engineering friends to accept their judgement that it is a good practical system.

Much of the difference between Chalmers and myself lies in fundamental approach. Ought one to aim, in teaching physics, to start off with a set of postulates that provide the neatest possible solution at the end ? Or ought one to start from observable physical facts, building up separate theories for each, and showing, with perhaps some awkward connexions, how these theories tie together to form a complete whole ? That I favour the latter is partly my own idiosyncrasy, but I am sure that there are many more physicists like me.

Let us consider magnetism, for instance. Students can handle magnets, plot fields and check them against calculation, assuming point poles. Yes, I know that point poles do not exist, but they are a good approximation—as good as many others in physics. Physicists must, in fact, learn how to handle approximations ; they form part of their stock-in-trade. As a physicist in a technological institution, I have to stand up to frequent jibes about physicists' idealized concepts ; my answer is that I am not ashamed of them ; I am proud of them ! After all, they have not served physics so badly. Certainly I do not relish having to explain that the earth's magnetic field can be measured in ampere turns per metre.

I must admit of some surprise at what seems to be a resuscitation of the aether, for that is how Chalmers' and Noakes' ideas seem to me. Do physicists in general accept the allotment of measurable properties to free space ? Am I out of date ?

I should like to say that I have received a dozen letters altogether, and have come to the conclusion that Institute members can be broadly divided into two groups—those who disagree with me and write to the Institute, and those who agree with me and write to me ! The score so far is 9-3. Mr. Macfadyen, of Birmingham University, has kindly sent me a copy of an article he has published in the *School Science Review*, June, 1954 ; it is a much abler—and more definite—article than mine and I strongly recommend it to those interested in the subject.

Finally, I deplore the last sentence of Chalmers's reply. Are traditional methods necessarily hide-bound ?

The 'Clock Paradox' *

(A REPLY TO PROFESSOR H. DINGLE)

By GEOFFREY BUILDER, D.Sc., F.Inst.P.

(*University of Sydney—Australia*)

PHYSICISTS who have made a special study of the current theory of relativity, or who are continually verifying its predictions in the course of their daily work, are unlikely to take seriously Professor Dingle's distortions of the theory. However, interest in relativity is widespread. Physicists and others who lack the time for special study of the subject must, of necessity, depend largely on the guidance of specialists and they may be seriously misled by any misinterpretation of the theory if it is emphatically presented in well-written articles by one so eminent.

The non-specialist reader might well be encouraged to adopt a somewhat sceptical attitude by Dingle's own enumeration of other specialists with whom he disagrees, i.e. Einstein (*Nature*, **177**, p. 784 : " That paper contains a most regrettable error, in a statement that a clock moving in a closed curve will be found, on returning to its starting point, to be behind a stationary clock "), Eddington (*Bulletin* **7**, p. 316 : " It was a fatal error "), all modern physicists (*Nature*, **177**, p. 783 : " The wholesale abandonment of even elementary reasoning which physicists have allowed themselves to sanction "), and mathematicians (*Nature*, **178**, p. 681 : " But what is needed here is not mathematics but a faculty for refusing to be bemused by mathematicians ").

On the other hand, Dingle has always been able to quote the " clock paradox " as a plausible reason for doubting the reliability of the predictions of relativity theory about the behaviour of clocks.

It is not always understood that the term " paradox " does not properly refer to the relativity theory prediction that one clock may, under certain circumstances, become retarded relative to another. The predicted retardations are not, as such, any more surprising or paradoxical than other remarkable consequences of the theory, such as the relativity of simultaneity, the reciprocity of measured vibrations of length and mass, the equivalence of mass and energy, and so on.

The " paradox " properly refers to an ambiguity, which has arisen in the calculation of such retardations by means of the restricted theory, and which has resulted in an appearance of paradox. It is well known that the general theory predicts precisely the same retardations without any ambiguity and without any appearance of paradox.

Moreover, I have recently shown[1] that the so-called paradox can be resolved completely within the framework of the restricted theory. Properly applied, this theory gives a unique and unambiguous value for the relative retardation of two clocks as a result of their motions. The " paradox " had arisen only through careless use of the " rate " of a moving clock without due regard to the exact significance of the quantity so described.

In particular, if one clock R is unaccelerated and another identical clock M (e.g. carried by Dingle's space traveller) performs a journey away from and back to R, a retardation of the clock M relative to clock R at the end of M's journey is an irrefutable and unambiguous prediction of the restricted theory taken together with the assumption, which is generally accepted and which is basic in the general theory, that the rate of a clock is a function only of its speed and not of its acceleration. In general, the path followed by M will be in the form of a closed curve when viewed

* Based on a lecture given to the New South Wales Division of the Australian Branch of the Institute.

from a reference system in which R is at rest. The mathematics involved in all this is of no more than high-school standard.

Thus the "paradox" may be dismissed as a source of uncertainty about the reliability of such a prediction. *The prediction can be contested only by denying the validity of current relativity theory.* The restricted theory can be stated thus :

> "Measurements made in different *inertial* reference systems (using measuring devices that would be identical if compared at rest together) are related by the Lorentz transformation."

This statement is valid whether one regards the theory as that of Poincaré and Lorentz (Whittaker, *History of the Theories of Aether and Electricity*, 1900-1926) or as that of Einstein (*Ann. der Phys.*, 1905); it is, in any case, the basis of the many current applications of the theory.

The *inertial* systems of reference mentioned in this statement are those which are unaccelerated and are free of gravitational fields. Einstein defined them clearly in his first relativity paper (*Ann. der Phys.*, 1905) as systems "in which the equations of Newtonian mechanics hold good, i.e. to a first approximation."

McCrea (*Nature*, **177**, p. 784) has previously pointed out that the basic fallacy in Dingle's point of view lies in his failure to recognize this restriction to *inertial* systems and he drew Dingle's attention to the fact that Einstein had stated the restriction in his 1905 paper. Dingle (*Nature*, **177**, p. 785) has simply denied that he could find it in the paper ; but it is there for all to see in the first sentence of the first section of Part I of that paper.

That this is indeed the essential fallacy in Dingle's thinking can be verified by reference to his discussions (*Bulletin*, **7**, pp. 314, 315, 320 ; *Nature*, **177**, p. 783) in which he makes it clear that *his* "fundamental principle of relativity," in its application to two bodies in relative motion, is that the motion is a relation between them and not something belonging to one or the other ; that either of the bodies may be at rest or may have any motion we care to assign to it ; that all the effects of the motions must apply equally to both bodies since it is only the relative motion which has observable effects.†

His "fundamental principle" is obviously incompatible with the concept of inertial reference systems in the restricted theory of relativity. For the theory not only requires that the relative motion of any two such systems should be uniform (and should, in principle, have been so, and continue to be so, indefinitely) but requires also that each such system should be unaccelerated.

Nor is it compatible with our general physical knowledge. For example, the accepted laws of electrodynamics show that the forces experienced by two charged particles in relative motion cannot be expressed solely in terms of their relative velocities and accelerations, but depend also on their individual motions. According to the restricted theory, these laws hold in every inertial reference system and all inertial observers must agree that the effects are not determined solely by the relative motion ; Dingle is a lone dissentient.

It is also well known, even to the newest graduate in physics, that the electrodynamical forces between two particles, resulting from their motions, are not always symmetrical. In general, the force exerted by the first on the second differs from that exerted by the second on the first, and Newton's third law is not satisfied. This is wholly incompatible with Dingle's assertion that all the effects of the motions must apply equally to both.

The source of Dingle's difficulties is his belief (*Bulletin*, **7**, p. 314) that :—

> "The whole theory of relativity, special and general theory alike, is based on the postulate that motion is a relative phenomenon."

He takes this to mean that acceleration has no significance except as acceleration of one body relative to another.

The restricted theory forbids this interpretation ; the Lorentz transformation does not hold between measurements made in two reference systems which are moving with uniform velocity relative to one another in a region free of gravitational fields but which are subject to a common acceleration.

Nor is there anything in the general theory to support this interpretation. What this theory does say is quite different. It states that the effects of acceleration are indistinguishable from the effects of a gravitational field, so that an observer cannot determine whether the effects he detects are due in fact to the acceleration of his system or are due in fact to the presence of a gravitational field. Thus he cannot measure absolutely his own acceleration or that of any other body. This is the famous principle of equivalence. Were there no detectable effects of acceleration there would be no such principle and, indeed, no general theory.

In this interpretation Dingle has completely ignored the fact, universally accepted in physics and in common sense, that acceleration causes detectable consequences which are, by and large, independent of the presence and motion of other bodies. In effect he ignores the accepted fact that material bodies have a property of inertia which is, as such, independent of the presence and motions of other bodies. McCrea (*Nature*, **178**, p. 681) has pointed out that " Dingle would not be symmetrical with a space traveller who turns into a corpse " as a result of the violent accelerations to which he has been subjected !

Thus Professor Dingle's interpretation of the basic principles of relativity theory must be dismissed as wholly inconsistent with our present physical knowledge. It would therefore be futile to enter into any debate on the detailed inferences he draws from his interpretation. One can only suggest that students seek a more reliable guide to an understanding of relativity theory and its consequences.

Professor Dingle comments :

Dr. Builder seems a little heated. It is a fair deduction—not unsupported—from his first sentence that he has not made a special study of relativity. I will deal chiefly with his main point, and briefly with the less trivial of the others.

Dr. Builder states that my " essential fallacy " is my statement of the principle of relativity. " The source of Dingle's difficulties " (why difficulties ?) " is his belief that ' the whole theory of relativity, special and general theory alike, is based on the postulate that motion is a relative phenomenon '." Let me refer to Einstein's original papers‡ in which he set out the two theories. In the first paragraph of the first paper he cites a number of phenomena associated with the relative motion of two bodies, which remain exactly the same whichever of the two bodies is the " moving " one, and he proceeds§ : " Examples of this sort . . . suggest that the phenomena of electrodynamics as well as of mechanics possess no properties corresponding to the idea of absolute rest. They suggest rather that . . . the same laws of electrodynamics and optics will be valid for all frames of reference for which the equations of mechanics hold good. We will raise this conjecture (the purport of which will hereafter be called the " Principle of Relativity ") to the status of a postulate." Nothing whatever has yet been said about measurement or the Lorentz transformation or anything of the kind : the Principle of Relativity is a purely qualitative statement that there are no phenomena in nature corresponding to the idea of absolute rest—or, as I stated it, that motion is a relative phenomenon. But *after* this, Einstein goes on to " introduce another postulate, which is only apparently irreconcilable with the former, namely, that light is always propagated in empty space with a definite velocity c which is independent of the state of motion of the emitting body." It is the addition of this postulate that gives rise to the Lorentz transformation and the mathematical part of the theory, the justification of which is that it conforms to the Principle of Relativity. If the second postulate were *actually*, instead of *apparently*, irreconcilable with that principle, it would not have been proposed.

The procedure is exactly the same with the general theory. In the 1916 paper Einstein begins by saying that " The special theory of relativity is *based on* (my italics) the following postulate " (postulate substantially as before). " This postulate we call the ' special principle of relativity '." He then goes on to

say that "the special theory of relativity does not depart from classical mechanics through the postulate of relativity, but through the postulate of the constancy of the velocity of light *in vacuo*, from which, in combination with the special principle of relativity, there follow . . . the Lorentzian transformation . . ." The phraseology is a little confusing, but no one who reads the passage can doubt that he is talking about a theory (let us call it "the special *theory* of relativity") which comprises first and foremost a *postulate* of relativity, and secondly a postulate of the constancy of the velocity of light which gives results conforming to the postulate of relativity. He then proceeds to extend the postulate of relativity (not the other postulate), and after giving an example of accelerated motion (corresponding to the earlier examples of uniform motion) he proceeds: "Of all imaginable spaces R_1, R_2, etc., in any kind of motion relatively to one another, there is none which we may look upon as privileged *a priori* . . . *The laws of physics must be of such a nature that they apply to systems of reference in any kind of motion.* (Einstein's italics.) Along this road we arrive at an extension of the postulate of relativity." Only *after* this does he say: "It will be seen from these reflections that in pursuing the general theory of relativity we shall be led to a theory of gravitation," and he then goes on to give his theory of gravitation.

It is clear, therefore, that the fundamental thing in both theories is *the postulate of relativity*, which is a simple statement that nature provides no phenomenon corresponding to the idea of absolute rest or absolute motion—not merely no metrical phenomenon, but no phenomenon at all. The Lorentz transformation and the principle of equivalence result from subsidiary postulates which are substituted for the Galilean transformation and Newton's gravitational force because they, and not the others, conform to the postulate of relativity.

It therefore follows inescapably that if one calculates from either of those subsidiary postulates that a phenomenon is possible that *does* distinguish which of two bodies is moving, then either the calculation is wrong or the subsidiary postulates are discredited. The asymmetrical ageing of twins under discussion would be such a phenomenon; the twin that ages slower is the one that is moving. I say that the calculation leading to this conclusion is wrong. Dr. Builder must either agree or scrap Einstein's theories of relativity.

It is the curse of our time that when once a mathematical formula has been derived, the grounds on which it rests are forgotten and the mathematical consequences of the formula are developed as though it were itself a necessary truth. We then get consequences, that plainly contradict the original premises, passionately advanced as deductions from those premises. The history and philosophy of science is a subject in ill repute; its students are spoil-sports. When a mathematician submits a physical paper to a society, the bases on which his formulae rest are not considered; only the process of deduction is examined, and if anyone ventures to question the conclusion he is told he is not a mathematician and he shouldn't be tiresome. I say this not to insult mathematicians, who are as God made them. I say it because it is true and needs to be said. This controversy is but one of a number of examples.

I apologize for taking up space about McCrea's charge that my summary of Einstein's special theory was a travesty because I had not said that the theory was restricted to "inertial frames of reference." This is a red herring, but it has been trailed so much that it must be attended to. McCrea in a letter to me had defined an inertial frame as "a freely moving rigid body," and he had told me in conversation that if that body was freely moving in a gravitational field it was still an inertial frame in his sense of the term. I said, correctly, that there was no reference to such a thing in Einstein's 1905 paper. He then pointed to the sentence in that paper introducing the "definition of simultaneity" which says: "Let us take a system of co-ordinates in which the equations of Newtonian mechanics hold good."

I could hardly know that McCrea's definitions were so fluid, but the whole point is entirely trivial. When I summarized Einstein's theory I did not mention this sentence because it was already implied in the whole discussion. It occurs at the beginning of the deduction of the Lorentz transformation, and that deduction is therefore subject to it. But McCrea had already used the Lorentz transformation in deducing his result : he was therefore regarding the problem as one in which Newtonian mechanics held good, or, to use his other phraseology, as one to which inertial frames were applicable. So it seems that he may use the special theory to get his result, but I must not use it to criticize that result. I hope we shall hear no more of this absurdity.

Dr. Builder has a different view of " inertial frames " : they are " unaccelerated and are free of gravitational fields." But in the case in question, if M is accelerated with respect to R, then clearly R must be accelerated with respect to M. If, then, R, but not M, remains in an inertial frame, M must have some acceleration other than a relative one. But Dr. Builder goes on to say that, according to the principle of equivalence, " an observer . . . cannot measure absolutely his own acceleration or that of any other body." What is this acceleration which is neither absolute nor relative to the only other body in the system ? Will Dr. Builder not tell us ? This is not a rhetorical question ; the whole of the supposed asymmetrical ageing depends on it. I await the answer with much interest.

Dr. Builder exaggerates in saying that " It is well known that the general theory predicts precisely the same retardations." I have not met anyone who knows it. A reference would be interesting. I have seen his paper in which the paradox is " resolved completely within the framework of the restricted theory " : the fallacy in it is obvious. I must demur from his statement that I have " always been able to quote the ' clock paradox ' as a plausible reason for doubting the reliability of the predictions of relativity theory." I do not at all doubt those predictions ; I simply differ from him as to what they are.

It is necessary to deal more fully with the serious innuendo that I have treated with disrespect those great men with some of whose views I disagree. I have never ceased to marvel at the insight of Einstein which enabled him to penetrate to the root cause of the dilemmas of his time and bring to light what is perhaps the most fundamental of all physical principles. I have no less admiration for the insight of Eddington, which gave him an understanding of that principle, when once it had been unearthed, which I believe to have been even deeper than that of Einstein himself. I have not failed, whenever occasion has called for it, to express the feelings, only just this side idolatry, which I have always entertained towards these thinkers to whom my debt is immeasurable. But while in science one may have guides, one has no right to admit authorities other than experience and reason, and if Dr. Builder would read a little of the history of science he would see that the greatest are particularly prone to error concerning the very subjects on which they

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* A paper entitled " The Resolution of the Clock Paradox " will be published in the June number of the *Australian Journal of Physics*.

† In one place he speaks more specifically of bodies in " uniform relative motion " ; but it is clear that he is not then using the word " uniform " in the accepted physical sense, for he is referring to the relative motion of two clocks which he describes as " separating and reuniting with uniform relative motion." It need hardly be said that two bodies continuing in uniform relative motion, in the accepted sense, can never reunite once they have separated.

‡ *Ann. d. Physics*. **17**, 891 (1905) ; **49**, 769 (1916).

§ I quote throughout from the English translation in *The Principle of Relativity* by W. Perrett and G. B. Jeffery (Methuen, 1923).

‖ *Naturwiss* **6**, 697 (1918).

†† M. Born. *Einstein's Theory of Relativity*, English translation, Methuen (1924), pp. 216, 282.

R. C. Tolman. *Relativity, Thermodynamics and Cosmology*. Oxford University Press (1934), p. 195.

throw most light. Galileo was wrong about the tides, Newton was wrong about falling bodies, Doppler never understood the application of his principle to stellar spectra ; and so on. One can well believe that the vision of a new light, unperceived before by anyone, can at first blind the eyes of its discoverer to details readily discernible by lesser men years afterwards. One does no service to the pioneers, and certainly none to science, by regarding their every word as infallibly inspired.

I hope that Dr. Builder will on reflection agree with me in this. In case not, however, I might point out that his statements that " the so-called paradox can be resolved completely within the framework of the restricted theory " and that " the assumption . . . is generally accepted and . . . is basic in the general theory, that the rate of a clock is a function only of its speed and not of its acceleration " are contradicted by Einstein.‖ They are contradicted also by M. Born and R. C. Tolman—names " tolerably known in the Revolution "—in their well-known books†† on the subject. It is, after all, better to stick to experience and reason rather than to personal authorities : they always win in the end.

Reviews of Books

MATHEMATICS FOR ELECTRONICS, WITH APPLICATIONS

By H. M. Nodelman and F. W. Smith

London : McGraw Hill Publishing Co. Ltd. Pp. 391 + viii. Price 52s. 6d.

The authors of this book have carried out a considerable research in the technical periodicals devoted to communications and electronics to determine (1) which mathematical topics are of greatest importance, and (2) to obtain more than 300 specific applications which have been incorporated into the text. As a result they claim to have produced a book in which the mathematical topics are thoroughly integrated with electronic engineering problems " to provide a maximum of reader motivation."

A background of elementary calculus, physics and electrical network theory is assumed. The book is in six parts, the main ones being Part 2 on units and dimensions including dimensional analysis ; Part 3 on the algebra of circuit analysis dealing with determinants and matrices ; Part 4 on series, mainly Taylor and Maclaurin expansions ; and Part 5 on differential equations, mainly linear equations with constant coefficients using both classical and Laplace transform methods. Each of these parts has a chapter of mathematics followed by chapters containing solutions of many examples from electronic engineering and problems for the student to solve, with frequent references to the papers from which the problems have been taken.

It contains a number of tables mainly concerned with electronics (e.g. matrices of simple four poles) though one is of Laplace transforms.

One gets the impression that the authors of this book rather resent the increasing intrusion of mathematics into electronics and have set out to determine the smallest amount of mathematics which will enable the types of calculation which arise in electronics to be carried out. The mathematical theory which is actually proved is very small and might be regarded as the minimum which would enable the student to appreciate sufficiently the meaning of the many theorems and rules which are stated without proof, to be able to apply them to electronic problems.

There is no attempt to teach mathematics, and even in the extensive sections on networks the book is only concerned with problem solving, theorems being stated when required without proof. Teachers might find it a useful source book for problems, but most are too specialized for use with any but electronics students.

No answers are given to the problems. The book contains a considerable number of misprints, and some errors. On p. 91 a theorem on the addition of two determinants is incorrectly generalized. On p. 263 the final step in the solution of a differential equation exemplifies two of the commonest errors of beginners. On p. 270 it is stated that the particular integral corresponding to a term ax^n on the right of a linear differential equation with constant coefficients may be obtained by using as a trial solution $Ax^n + Bx^{n-1}$, which is not correct if $n > 1$. On p. 182 the transfer matrix of a π-network is incorrectly stated, and the error is repeated on the opposite page.

The pictures at the beginning of each part have no relevance to the text ; and as every student should have his own book of mathematical tables, it seems quite unnecessary to include such tables in textbooks.

It seems a pity that the results of so much effort should not have been better used.

A. W. GILLIES.

WAVE OPTICS (INTERFERENCE AND DIFFRACTION)

By C. Curry

London : Edward Arnold Ltd. Pp. 155+viii. Price 21s.

It is not uncommon for writers on wave optics to treat the subject as a set of exercises in the analysis of fascinating, but useless phenomena. Dr. Curry, on the contrary, has a more practical objective clearly in mind from the outset. Wave optics expresses more of the truth about the propagation of light than geometrical optics, and is therefore necessary for an understanding of optical systems. " Geometrical," no less than diffractional defects of images are here treated on the basis of wave theory. Since the consequences of using finite beams of light are discussed at the outset, it is natural for the author to treat diffraction before interference. After the principles of interference have been developed it becomes possible to deal with the formation of images when objects are illuminated by coherent light. This refreshing approach is successfully developed and culminates in a discussion of useful applications of wave optics.

Dr. Curry's book is a text for undergraduates, a successor to his earlier volume on Geometrical Optics. It is not highly sophisticated, nor does it use any more advanced mathematical functions than sine and cosine. It is intended for the first and second post-intermediate years of a General course, or for the first year of a three year Special Physics course. Perhaps the author has been rather modest in defining his public in this way, for, as far as it goes, the text will satisfy more mature undergraduates. The limitations of the book lie in the selection of subject matter rather than in narrowness of outlook on what is treated. Thus, as the sub-title implies, polarization and double refraction are excluded, and there is no attempt to discuss from the atomistic point of view the interaction of light with matter. Kirchhoff's theory of diffraction is mentioned but not discussed.

In a book of 150 pages on this subject, an author is called upon to be selective. Dr. Curry has chosen to give extended discussions of difficult points of principle rather than to elaborate on the detail of experimental techniques. For example, his treatment of the location of thin-film interference fringes is considerably more complete than one finds in other books of this standard. Likewise, he devotes a whole chapter to the Abbe theory of image formation and its application in phase-contrast microscopy. A list of thirty references guides the reader to more elaborate treatments of topics mentioned in the text, and a number of examples, mainly numerical, test the student's mastery of the subject.

Some teachers will regret that transmission and reflection echelons have been included with the Fabry-Perot etalon and the Lummer plate in a chapter on Interferometers, rather

than in the chapter on Gratings. The classification is not entirely a matter of taste. On a point of omission, it is surprising that the author has not illustrated with reference to the prism and the grating the well-known relation that the resolving power is equal to the product of the width of the beam and the dispersion. Such an observation would have been in keeping with the spirit of the book. On a further minor point, in connection with the fringes formed by a Fabry-Perot interferometer, simplification of formulae would have resulted from a definition of resolving power based on the separation of maxima by the half-intensity width of the fringes, rather than on the criterion of 19% dip.

The book is priced reasonably for to-day. It is well printed and the diagrams are clear. It can be recommended as a concise and valuable text in its own right, and one which, moreover, should stimulate the intelligent undergraduate to make good use of the list of references.

G. W. Series.

ELEMENTS OF PURE AND APPLIED MATHEMATICS

By H. Lass

London : McGraw Hill Publishing Co. Ltd. Pp. 491 + xi. Price 56s. 6d.

The term " applied mathematics " in the title of this attractive book is not used in the traditional English way. The book is devoted entirely to particular sections of pure mathematics which arise in physics and engineering and have been applied, increasingly in recent years, in the solution of problems in these fields.

Successive chapters provide introductions to such topics as matrices, vectors, tensors, complex variable theory, differential equations, orthogonal polynomials, Laplace transforms, group theory and probability theory. These may be found, in spite of the clarity of the writing, far from easy but there are useful bibliographies to guide those who would read more widely and deeply.

The last chapter on real variable theory begins with a sketch of the author's philosophy of mathematics and science. He suggests that the scientist " is prone to believe that the laws of nature are discovered," and adds " it is our personal belief that this is not the case. We lean towards the more esoteric point of view that the so-called laws of nature are invented by man." This leads to a discussion of the positive integers from carefully defined postulates, followed by an introduction to the theory of sets ; the more familiar work on differentiation and integration of functions of a real variable is then developed.

The exposition throughout is lucid and attractive, but the author is too optimistic in claiming that " this book can be used to teach almost any undergraduate mathematics course from the junior level onwards." One hopes, however, that he is right in thinking that " the discriminating reader should enjoy the rigor of analysis which is missing from most applied mathematics texts." A few physicists at least should find this Lass not too forbidding.

J. Topping.

REALITIES OF SPACE TRAVEL

Edited by L. J. Carter

London : Putnam & Co. Ltd. Pp. 431. Price 35s.

Whether one agrees or not with the Astronomer Royal about the future of space travel, serious discussion of the question certainly raises some very pretty problems, in chemistry, physics, astronomy and physiology, to say nothing of politics, It is the purpose of the present volume to survey the scientific problems involved, to explain how they are being attacked at present and to indicate the lines along which solutions may eventually be found. It consists of 24 papers, by 16 authors, contributed at various times to the proceedings of the British Interplanetary Society. They are grouped according to subject, and cover the whole field very satisfactorily, with comparatively little overlap of material.

The chapter of most immediate interest, in view of the American (and Russian ?) I.G.Y. project, is that on the satellite vehicle. As none of the three papers on this subject is of later date than 1953 they do not include

recently published details of the proposed satelite, but the general principles with which they are concerned are nevertheless of much interest. The facts presented imply that an unmanned satellite should be immediately practicable, and that the difficulties attending the return to earth of a manned one are probably not insuperable. Beyond this point, that is to say so far as visits to the Moon and the planets are concerned, progress is at present impossible by reason of the non-availability of a sufficiently energetic propellant. Various alternatives are surveyed, the most favoured being nuclear energy and the ion gun, but practical applications of these are by no means in sight as yet. Of the physiological effects associated with space flights that of weightlessness is probably the one of most general interest, and this receives a particularly full discussion, which leads to the conclusion that it is unlikely to be a major difficulty.

The Moon and Mars, as priority destinations, each have a special section describing the nature of their surfaces and the conditions obtaining thereon. Both are extremely readable and informative. There is also a very thoughtful article on the wider, largely political, aspects of the space travel project. Even the possibility of interstellar travel is seriously discussed, and, for full measure, accounts are given of the American and Australian rocket testing establishments and their equipment. Of special interest to those of us who observed the V2 missiles from the target area is a history of the development of these weapons, written by Major-General Dornberger, who was for many years engaged in such work, and was eventually given the responsibility for the German guided missile programme.

The book can be unreservedly recommended to everyone interested in any of the aspects of space travel.

W. E. CURTIS.

The Institute's Journals

PRINCIPAL CONTENTS OF THE JUNE, 1957, ISSUES

Journal of Scientific Instruments: Volume 34, No. 6

SPECIAL ARTICLE

Instrument transducers. By J. Thomson, F.Inst.P. (*British Scientific Instruments Research Association, Chislehurst, Kent*).

After dividing the problems of instrumentation into two categories, (1) the determination of what is to be measured and (2) the investigation of how to make the measurement, the importance of energy transformations to problems in the second category is established. The attempt is made to define the forms of energy in a manner suitable for instrument research and a chart is presented showing important physical and chemical phenomena as transformations between these forms of energy. Particular attention is paid to those transducers which provide an electric signal. In conclusion, recent developments of transducers are considered from the point of view of instrumentation and the significance of solid state science is underlined.

ORIGINAL CONTRIBUTIONS

A water-cooled mercury arc lamp for the excitation of Raman spectra. By L. A. Woodward and D. N. Waters (*Inorganic Chemistry Laboratory, University of Oxford*).

A modified Toronto-type mercury vapour lamp, suitable for the excitation of Raman spectra, is described. It is constructed throughout of Pyrex and is water-cooled, not only at the electrodes, but also along the whole length of the arc (approximately 185 cm). An inverted-U shape is adopted, so as to allow free expansion of the inner arc tube relative to the cooling jacket and thus render expansion bellows unnecessary. Current-voltage characteristics are given for the range 10-32 A. The intensity of emission of each of the mercury lines 4046, 4358 and 5461 Å is determined by photographic spectrophotometry as a function of

lamp current or wattage. For a given current the light output is found to be slightly lower when the lamp is running with full cooling than when it is running as an ordinary Toronto arc with cooling of the electrodes only; but with full cooling higher currents can be used and thus greater light outputs attained. However, the rate of increase of intensity with increase of current is found to fall off at higher values; so that, for a given wattage, a single lamp gives less light than several in parallel.

An apparatus giving thermogravimetric and differential thermal curves simultaneously from one sample.

By D. A. Powell (*Division of Building Research, Commonwealth Scientific and Industrial Research Organization, Melbourne, Australia*).

A description is given of modifications to a Chevenard thermobalance which enable it to record simultaneously, but on separate charts, both thermogravimetric and differential thermal curves of a single sample undergoing thermal decomposition. A method of placing temperature marks on both charts is also described. Some records obtained and the performance of the apparatus are briefly discussed.

Single crystal oscillation photographs about three crystallographic axes from a single setting on a Weissenberg camera.

By J. Shankar and P. G. Khubchandani (*Atomic Energy Establishment, Trombay, India*).

The paper gives the design of a cam and its mounting to enable oscillation photographs to be taken about the three crystallographic axes from a single crystal setting. The modification is particularly useful with crystals of radioactive substances or those having undeveloped faces.

An automatically recording vacuum balance.

By M. I. Pope (*Washington Singer Laboratories, The University, Exeter*).

The apparatus described can record changes in weight of a solid, either in the presence of a gas or *in vacuo*, at temperatures of up to 1,000°C, to an accuracy of ±0·2 mg with a total range of approximately 5 g. Simplicity in construction is achieved by use of only standard radio and electrical components. Applications include thermogravimetric analysis, gas sorption and studies of gas-solid reactions.

Optical and instrumental limitations to the accuracy of "oximetry."

By R. H. Kay, A.Inst.P., and R. V. Coxon (*The University Laboratory of Physiology, Oxford*).

An "oximeter," intended primarily for measuring colorimetrically the oxygenation of circulating blood in living tissues, has been tested on blood contained in glass absorption cells. The instrument reading is shown to depend not only on oxygenation but also on the total amount of pigment (haemoglobin) in the light path, and thus a single calibration curve is only acceptable if this amount is constant. Comparison of the optical density of whole blood with a sample in which the haemoglobin has been released from the corpuscles into homogeneous solution reveals the large contribution to the total light extinction which derives from scattering effects in whole blood. This factor, together with instrumental deficiencies, limits severely the accuracy of measurements with this type of oximeter on blood within the circulation, and the accuracy of the method on whole blood in glass absorption cells is estimated as ±8% oxygen saturation.

A solid state paramagnetic resonance spectrometer.

By P. M. Llewellyn (*Clarendon Laboratory, University of Oxford*).

A spectrometer is described for use in the frequency range 9,000 to 10,000 Mc/s and at the fixed temperatures of boiling refrigerants and a small range below obtained by pumping on the liquid. The lowest temperature available is the triple point of hydrogen (14°K). A high sensitivity is achieved by modulating the magnetic field at 115 kc/s. The sensitivity is such that 10^{13} atoms of ^{56}Co have been detected at 20°K with a signal/noise ratio of 3 : 1; it is estimated that 2×10^{-11} molecules of the free radical diphenyltrinitro phenyl hydrazyl (DPPH) would be detectable at 290°K.

An evaporated gold bolometer.

By E. Archbold (*National Physical Laboratory, Teddington, Middlesex*).

The construction of a bolometer is described in which the element—of evaporated gold supported by a thin plastic film—is 2·6 mm long, 0·2 mm wide and has a resistance of 15 Ω. In vacuum it has a sensitivity of 1·5 V/W and a minimum detectable energy of $3{\cdot}6 \times 10^{-10}$ W, both measured at 10 c/s. The time constant in vacuum is 7 ms.

The design of calorimeters for the determination of ion beam intensities.

By E. R. Harrison, A.Inst.P. (*Atomic Energy Research Establishment, Harwell, Berks*).

Calorimetric methods for determining the total particle flux of ion beams are discussed. Three types of calorimeter are described which, from experience gained with 25-50 mA, 10 keV and 0·2 mA mean, 500 keV proton beams, have proved suitable for this purpose. The sensitivity of the calorimeters can be either calculated from elementary theory, or obtained directly by a convenient method of calibration.

British Journal of Applied Physics: Volume 8, No. 6

SPECIAL ARTICLE

Physics below 1°K. By E. Mendoza (*H. H. Wills Physical Laboratory, University of Bristol*).

A temperature T can only be called "low" if kT is small compared with some energy level spacing characteristic of the system. On this criterion 1° K is often not a low temperature, and the region below 1° K is one in which many events occur. The adiabatic demagnetization of paramagnetic bodies is the commonest technique for attaining these low temperatures, though other methods also exist. The main limitation on experiments is the small amount of power which can be absorbed by the working substances. At temperatures down to 3×10^{-2}° K, a number of experiments have been performed, not only on the refrigerants but also on other substances. Results from several branches of physics, including solid state and nuclear physics, are briefly presented and discussed. Below 3×10^{-2}° K only the refrigerants have been investigated; the lowest temperature yet reached is about 2×10^{-5}° K.

ORIGINAL CONTRIBUTIONS

The measurement of surface residual stress by X-rays. By G. A. Hawkes (*The Fairey Aviation Co. Ltd., Hayes, Middlesex*).

A multi-exposure, X-ray, back-reflection technique, with graphical solution for the evaluation of the level of residual macro-stress in any particular surface direction in a polycrystalline material, is described. This technique has, in particular, been applied to the stress analysis of aluminium alloys and high tensile steel components and specimens.

The heating of fluorescent screens bombarded by electrons. By G. D. Archard, A.Inst.P., and P. A. Einstein (*Associated Electrical Industries Ltd., Aldermaston, Berks*).

The temperature rises of fluorescent screens under various forms of electron bombardment (such as pulsed or scanned beams) are calculated in terms of screen dimensions and the thermal constants of the phosphor. Fair agreement is found between theory and experiment. Worked examples relating to practical cathode-ray tubes are given.

The penetration of irregularly-shaped particles through an airborne-dust elutriator. By J. G. Dawes, F.Inst.P., G. K. Greenough and J. S. Seager (*Safety in Mines Research Establishment, Ministry of Fuel and Power, Sheffield*).

A connection is established between the shape-factor distribution relating the projected-area diameters (D_M) and the Stokes' diameters (D_S) of particles in a dust cloud, and the penetration efficiency $E(D_M)$ of a standard elutriator for those particles determined by the microscope evaluation of thermal precipitator samples taken with and without the elutriator. From the measured characteristic $E(D_M)$ in a given dust cloud, it is possible to estimate the mean value of the shape factor D_S/D_M of the cloud, and its coefficient of variation.

Electron microscope and diffraction studies of 7% chromium-molybdenum steels. By Betty E. Williams (*National Physical Laboratory, Teddington, Middlesex*).

An investigation into the structural properties of some 7% chromium-molybdenum creep resisting steels has been carried out using electron microscopy and diffraction. The presence of fine precipitate of titanium carbide in good creep steels is observed.

The movement of the second crossover potential of insulators. By A. B. McFarlane (*Electronic Tubes Ltd., High Wycombe, Bucks*).

The secondary emission coefficient δ of metals and insulators has hitherto been considered to be a definite function of primary electron energy eV_p. The curve relating δ to V_p has two positions, one at low potentials in the range 10 to 100 V, and the other from 1,000 V upwards, at which δ is equal to unity, with a maximum value between them. These two positions are known as the first and second crossover potentials respectively.

In a cathode-ray tube, the luminescent screen is an insulator, itself mounted on an insulating glass base. The curve relating δ to V_p then determines the highest potential to which the screen can be brought, this being at or near the second crossover. This effect is very pronounced and is well known in earlier cathode-ray tubes. It sets a quite definite limit to the luminance available and also to the high voltage which can usefully be applied to the tube.

It has now been found that by suitable treatment of the screen, for example by settling it in a strong silicate solution or by applying a layer of magnesium oxide smoke to the bombarded surface, the second crossover may be made to vary rapidly with the electric field applied. In this way the second crossover can be made to follow closely the applied anode voltage, thus allowing the application of almost unlimited values to the tube, with a consequent gain in luminance and performance.

BRITISH THOMSON-HOUSTON

The British Thomson-Houston Co. Ltd., has a vacancy in the semiconductor group of its Research Laboratory at Rugby for a mathematical physicist. The work is concerned with the analysis of data obtained from the varied experiments on semiconductor materials and the devices made from them, and also the application of theoretical physics to the search for new semiconducting materials and devices.

The position is pensionable and offers excellent prospects.

Applicants are invited to write to the Director of Research, The British Thomson-Houston Co. Ltd., Rugby, stating age, qualifications and experience, and quoting reference WI.

PHYSICIST OR CRYSTALLOGRAPHER

Courtaulds Ltd. requires a Physicist or Crystallographer (man or woman), preferably with research experience and under 30, for the X-ray section of one of their new laboratories in Coventry. The work covers a wide range including fundamental structural problems on new fibres, such as polypeptide fibres, and single crystal work.

Good initial salary which is reviewed annually. The appointment is pensionable and there is a co-partnership scheme. Housing problems of a married man will be given sympathetic consideration and assistance in finding suitable accommodation will be extended to all.

Candidates should write for a detailed form of application to the Director of Personnel, Courtaulds Ltd., 16 St. Martins-le-Grand, London, E.C.1, quoting reference number D.76.

C.A.V. LTD.

Mathematician with honours degree required for theoretical work on combustion in diesel engines. This is a vacancy for a graduate following a career in applied mathematics, but experience of computer programming would be an advantage. Training in computer programming may be given to an otherwise suitable candidate.

Write stating age, qualifications and experience to the Personnel Manager, C.A.V. Ltd., Warple Way, Acton, W.3, quoting ref. R.32.

ILFORD LTD. require a graduate in Physics. A vacancy exists in the Company's research laboratories at Brentwood, Essex, for a young physicist to work on the design and development of special processes, machines and instruments and also to carry out investigations into the properties and applications of photographic materials. The post is best suited to a person with a preference for practical work and a flair for improvisation. Applicants should have an honours degree in physics and be free from National Service commitments. Apply in writing to the Research Director, Ilford Ltd., Ilford, Essex.

ILFORD LTD. require a University graduate (27 to 37) for research on the use of electronics in photography. A knowledge of television circuit techniques and the principles of colour television and photography will all prove useful. The essential requirements are a degree in physics or electrical engineering, experience in electronics and a flair for research directed at practical results. The successful candidate will work with a senior research engineer in the Physics Research Laboratory at Brentwood, Essex. Apply in writing to the Research Director, Ilford Ltd., Ilford, Essex.

COMMONWEALTH OF AUSTRALIA

COMMONWEALTH SCIENTIFIC AND INDUSTRIAL RESEARCH ORGANIZATION

DIVISION OF PHYSICS

Appointment No. : 770/116 of Research Physicists

The Division of Physics, National Standards Laboratory, located in the grounds of the University of Sydney, invites applications from physicists for experimental research in its low temperature group. Appointees will have some freedom of choice in research within the overall field of the study of low temperature phenomena.

The main work of the group in both theoretical and experimental research has been on the basic structure of solids, particularly metals and alloys, by examination of their specific heats, and electrical and thermal resistances. Investigations using paramagnetic resonance are also in progress. The facilities available include a virtually continuous supply of liquid helium, a 15,000 gauss magnet, and the assistance of technicians, a Drawing Office and fully equipped workshop.

Two positions are available. All applicants should possess a University honours degree in physics or equivalent qualifications and preferably have research experience to the Ph.D. standard. According to qualifications and experience the commencing salary will be determined within the ranges of Senior Research Officer £A2,048–£A2,323 p.a. and Research Officer £A1,313–£A1,938 p.a.

First class sea fares will be paid for the appointee and his wife and family. Appointment will be conditional on a satisfactory medical examination. If desired, appointment will be initially for a period of three years and if at the completion of this period the appointee does not take up a permanent position in C.S.I.R.O., return fares will also be paid. Alternatively, appointment can be for an indefinite period and the appointee will be required to contribute to and be eligible to receive benefits from either the Commonwealth Superannuation Fund or the Commonwealth Provident Account.

Applications, quoting Appointment No. 770/116 and stating full name, place and date of birth, marital status, nationality, present employment, particulars of qualifications and experience and of war service, if any, and the names and addresses of at least two referees and copies of not more than four testimonials, should reach the undersigned, from whom further particulars may be obtained by July 6, 1957.

A. Shavitsky,
Chief Scientific Officer.

Australian Scientific Liaison Office,
Africa House,
Kingsway,
London, W.C.2.

RESEARCH PHYSICAL METALLURGIST

An opportunity occurs for an honours graduate in metallurgy or physics having an interest in research on materials to join a small team in North Acton investigating long and short-term projects aimed at developing the industrial applications of the platinum-group metals. This is an interesting appointment in an unusual field and carries an attractive salary. Apply to The Manager, Development and Research Department, The Mond Nickel Co. Ltd., Thames House, Millbank, London, S.W.1. Mark envelope confidential PM.4.

ROYAL MILITARY COLLEGE OF SCIENCE

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The Civil Service Commissioners invite applications from men of high standing in their subjects for the following new professorships under the War Department :

(*a*) A Professor of Physics with special interests in nuclear science.

(*b*) A Professor of Engineering who may have special interests in one or more of the fields of heat engines, mechanics of fluids, mechanics of machines, materials and structures.

(*c*) A Professor of Electrical Engineering with special interests in radar and telecommunications.

Appointment in September, 1957, or as soon as possible thereafter.

The College prepares some 200 officers for degrees in engineering or science (mathematics, physics and chemistry) of the University of London. There is also a wide range of specialist courses at postgraduate level for more senior officers.

The professors are expected to engage in research and to advise on the researches of others. The instructional and research facilities are on an ample scale.

Age at least 35 years on August 1, 1957. Posts are pensionable. Salary : £2,300–£2,600. Starting salary above the minimum in approved cases.

Secondment from, and transfer to, the Scientific Civil Service may be arranged. The College is residential ; houses available at a rental for married men and quarters in residential officers' mess at a charge for single men. Further particulars including a statement about the college and a form of application may be obtained from the Civil Service Commission, Scientific Branch, 30 Old Burlington Street, London, W.1, quoting No. S4718/57/12. Application forms should be returned by June 26, 1957.

CITY OF PORTSMOUTH

COLLEGE OF TECHNOLOGY

ASSISTANT LECTURER (GRADE B)

Assistant Lecturer (Grade B) required in September to teach Physics and Mathematics.

Applicants should have a good degree or equivalent qualification.

Industrial experience would be an advantage.

Salary : Burnham Technical Scale £650–£1,025 (men) ; £580–£820, equal pay increments (women)—with graduate and training allowances and increments for professional or industrial experience where applicable.

Further particulars and form of application should be obtained from the Registrar as soon as possible.

E. G. Barnard,
Chief Education Officer.

RESEARCH ON DIELECTRICS.—Physicist, graduate, required in the research laboratories of British Insulated Callender's Cables Ltd. at Shepherds Bush. The work involves research on the dielectric properties of polymers and the development of improved methods for dielectric measurements. Knowledge of the dielectric properties of solids and of measurement techniques is desirable and keen interest in these fields is essential. Five day week. Pension Fund. Applications in the first instance, giving details of age, qualifications, experience and salary required should be made to Manager (Personnel), B.I.C.C. Ltd., Norfolk House, Norfolk Street, London, W.C.2, quoting reference W.L.4.

COMMONWEALTH OF AUSTRALIA

COMMONWEALTH SCIENTIFIC AND INDUSTRIAL RESEARCH ORGANIZATION

DIVISION OF ELECTROTECHNOLOGY

Appointment No. 760/94 of Research Officer

The Organization's Division of Electrotechnology, located in the University grounds, Sydney, is extending its research facilities at high voltages and invites applications from Physicists or Electrical Engineers to take part in a research programme for (*a*) the development of precision standards and methods of measurement and (*b*) the investigation of the behaviour of materials under high voltage stress.

Applicants should possess a University honours degree in physics or electrical engineering together with some postgraduate research experience. Dependent upon qualifications and experience commencing salary will be determined within the salary range of Research Officer £A1,287 – £A1,912 p.a. Salary for a female appointee would be £A160 p.a. less than the corresponding male figure.

The appointment will be conditional upon a satisfactory medical examination and the appointee will be required to contribute to, and eligible to receive benefits from, either the Commonwealth Superannuation Fund or the Commonwealth Provident Account. First class sea fares to Australia will be paid for the appointee, his wife and family.

Mr. Clothier, the senior member of the group is at present in the United Kingdom and those interested should obtain further particulars from him quoting reference No. 760/94, at the address given below. Applications referring to appointment No. 760/94 and stating full name, place, date and year of birth, marital state, present employment, particulars of qualifications and experience and of war service, if any, accompanied by copies of not more than four testimonials and the names and addresses of at least two persons willing to act as referees, if required, should reach the undersigned not later than July 6, 1957.

A. Shavitsky,
Chief Scientific Liaison Officer.
Australian Scientific Liaison Office,
Africa House,
Kingsway, London, W.C.2.

ESSEX EDUCATION COMMITTEE

SOUTH-EAST ESSEX TECHNICAL COLLEGE AND SCHOOL OF ART

LONGBRIDGE ROAD, DAGENHAM

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PHYSICIST, graduate, required for the research laboratories of British Insulated Callender's Cables Ltd., Shepherds Bush, London, W.12. The work involves research into properties and methods of preparation of ceramic materials used in assembly of resistors, capacitors and R/F power bodies. Special experience in ceramic technology is not essential but a knowledge of an interest in the properties and reactions of inorganic oxides, clays and refractories would be useful. Five day week. Pension Fund. Applications giving details of age, qualifications and experience should be made to Manager (Personnel), BICC Ltd., Norfolk House, Norfolk Street, London, W.C.2, quoting reference W.L.8.

PHYSICIST, graduate, required for the research laboratories of British Insulated Callender's Cables Ltd., Shepherds Bush, London, W.12. The work involves research and development of vacuum metallizing processes, instrumentation and measurement of electrical properties of capacitor components. Special experience in vacuum technology would be desirable but is not essential. Five day week. Pension Fund. Applications giving details of age, qualifications and experience should be made to Manager (Personnel), BICC Ltd., Norfolk House, Norfolk Street, London, W.C.2, quoting reference W.L.9.

PHYSICIST, graduate or equivalent, required for Research Department of The British United Shoe Machinery Company, Leicester, to work on physical problems in wide and expanding field including material and process development. The position offers good prospects for a man with initiative. Starting salary up to £1,100 per annum. The Company employs over 4,000 and makes a large range of machinery, mainly for use in the Boot and Shoe Industry, but also for the printing, leather goods. upholstery, pottery, radio and electrical industries, The Company has a pension scheme. Five day week. Housing difficulty recognized. Apply, giving full details, to The Staff Manager, Union Works, Belgrave Road, Leicester.

PHYSICS GRADUATE, or prospective graduate, man or woman, required for interesting work in the compounding research department of Dunlop Research Centre. The work, involving a classical physics background, would comprise fundamental studies in the modification of synthetic rubbers and other polymers by compounding techniques and gamma irradiation. Excellent facilities, five day week, and pension scheme. Apply in writing, giving details and salary required, to Personnel Manager, Dunlop Research Centre, Erdington, Birmingham, 24, quoting reference J.M.117.

The Institute of Physics

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PROCEEDINGS OF THE

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London 1954

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INDUSTRIAL, CHEMICAL AND OTHER APPLICATIONS

The volume is bound in cloth-covered boards, and is presented in the style and format of the Journal of the Royal Microscopical Society. The price is £4 10s. 0d., post paid, from the office of the Society, but £2 10s. 0d., post paid (one copy only), to registered participants in the 1954 Conference who have already placed their order. Orders should be placed with the Assistant Secretary, Royal Microscopical Society, Tavistock House South, Tavistock Square, London, W.C.1, or any bookseller.

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Appointments Vacant

The inclusion of particulars of a vacancy must not be taken to imply that the Institute necessarily approves of the terms offered or that might be offered. Candidates should satisfy themselves that these are satisfactory, and they may always refer to the Institute for advice.

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Research Physicist required to work on basic and development problems associated with a very wide range of materials and components used in the electronic industry. New, spacious and well-equipped laboratories are available and a large programme of basic research and interesting development work is entertained. This appointment will provide an opportunity for a member of the Institute to engage himself on a variety of problems involving many principles of modern physics, while the scope and further opportunities which exist within the expanding organization are very good. An invitation to inspect the laboratories, to ascertain the amenities provided and to discuss the important question of salary will be extended to selected candidates who forward very full details of their qualifications, interests and career to

The Personnel Manager,
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AUCKLAND UNIVERSITY COLLEGE
(UNIVERSITY OF NEW ZEALAND)

The College Council invites applications for two vacant Senior Lectureships in Physics. The Department is interested in various research fields, including angular correlation of nuclear radiations ; polarized nuclear reactions ; ion optics ; time-of-flight of neutrons. Preference would be given, other things being equal, to candidates with theoretical or experimental knowledge of any of the above fields ; or similar fields, combined with special qualifications as a lecturer in either (*a*) nuclear physics ; or (*b*) advanced electricity and magnetism.

The salary scale for a Senior Lecturer is £1,275–£1,575 per annum, by three annual increments. Commencing salary within this scale for the two physics posts will be fixed according to qualifications and experience. An allowance is made towards travelling expenses.

Further particulars and information as to the method of application may be obtained from the Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1.

The closing date for the receipt of applications, in New Zealand and London, is July 31, 1957.

THE UNIVERSITY OF SHEFFIELD

Three Research Bursaries are available in the Department of Fuel Technology and Chemical Engineering for graduates in Physics, Mathematics, Chemistry, Chemical Engineering, or other engineering subjects, who wish to enter the important field of flames and fuels. The work relates to heat transfer from flames and furnace aerodynamics. Value in the range £300–£500 per annum plus research fees. Bursars may become candidates for higher degree of the University. Applications should be sent to the Registrar as soon as possible.

COMMONWEALTH OF AUSTRALIA
AUSTRALIAN ATOMIC ENERGY COMMISSION

The Commission invites applications for the appointment of a Nuclear Scientist or Engineer for economic and technical studies of nuclear power developments in Australia.

A salary range within the limits £2,653–£3,550 p.a. (including cost of living adjustment) is contemplated. Further particulars are obtainable from The Australian Atomic Energy Commission Liaison Officer, Australia House, The Strand, London W.C.2. Applications close with the Secretary, Australian Atomic Energy Commission, G.P.O., Box 5343, Sydney, Australia, on July 22, 1957.

UNIVERSITY OF ABERDEEN

Applications are invited for appointment as Lecturer in Natural Philosophy (physics) for work in the Medical Physics Unit. Salary on revised scales expected to be in range £900 to £1,650. F.S.S.U. and children's allowance. Part removal expenses refunded.

Particulars from the Secretary, The University, Aberdeen, with whom applications (eight copies) should be lodged not later than August 31. Applicants outside the British Isles may submit one copy for application.

PHYSICIST or X-RAY CRYSTALLOGRAPHER with postgraduate research experience required for fundamental research work involving geiger spectrometer techniques. Some knowledge of metallurgy desirable but not essential. Attractive salary according to age and experience. Reply in writing to Research Manager, Murex Welding Processes Ltd., Waltham Cross, Herts.

PLESSEY NUCLEONICS LTD., have a vacancy for a Physicist whose interest lies in devising and carrying out basic experiments. The successful applicant will become a member of a team engaged upon designing new methods and instruments for nuclear reactor measurement and control. Considerable scope will be given to develop new and original ideas. The salary for this post will be on a generous scale depending upon previous experience and qualifications. Applicants should write in confidence giving full particulars to the Chief Engineer, Plessey Nucleonics Ltd., Weedon Road, Northampton.

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PHYSICISTS

to work on the following techniques :

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The work involves in the first place the establishment of apparatus and then its application to studies of molecular structure. These are new techniques of considerable potential value in an expanding scientific industry. The posts offer excellent opportunities for carrying out original research and the publication of results of scientific value is encouraged.

Applications are invited from Physicists having good Honours degrees, and preferably with some research experience and knowledge of the techniques involved.

These appointments carry attractive starting salaries which depend on age, qualifications and experience.

Please write, giving brief details of age, qualifications and experience to Staff Department, Hexagon House, Blackley, Manchester, 9. Quoting Reference I/13.

ELECTRICAL ENGINEER or PHYSICIST required for design and development work on electronic and similar instruments for use in coal mines and at the N.C.B.'s Mining Research Establishment Laboratories. The work covers a wide range, offers scope for originality since electrical instrumentation in the mining industry is a new field, and is carried out mainly at the Establishment though occasional pit visits may be necessary. Good honours degree or equivalent is required. Appointment (superannuable) according to qualifications and experience within inclusive scales as either Scientist 1 (£1,487–£1,964) or Scientist 2 (£1,059–£1,468). Good leave and sickness privileges, staff canteen, etc.
Write for application form to Personnel Officer, National Coal Board, Worton Hall, Isleworth, Middlesex marking envelope X/778/F before July 17, 1957.

BRITISH THOMSON-HOUSTON

The British Thomson-Houston Co. Ltd., has a vacancy in the semiconductor group of its Research Laboratory at Rugby for a mathematical physicist. The work is concerned with the analysis of data obtained from the varied experiments on semiconductor materials and the devices made from them, and also the application of theoretical physics to the search for new semiconducting materials and devices.
The position is pensionable and offers excellent prospects.

Applicants are invited to write to the Director of Research, The British Thomson-Houston Co. Ltd., Rugby, stating age, qualifications and experience, and quoting reference WI.

THE BRITISH THOMSON-HOUSTON CO. LTD.

The British Thomson-Houston Co. Ltd., has vacancies for physicists in a group carrying out investigations into the properties of semiconducting materials. The work will be mainly concerned with the physical properties of rectifiers and associated devices, and offers wide experience in both fundamental and applied research in a rapidly expanding field.

Applicants are invited to write to the Director of Research, The British Thomson-Houston Co. Ltd., Rugby, stating age, qualifications and experience and quote reference SJ.

PHYSICISTS

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The plastics industry is the most rapidly expanding " materials " industry and Bakelite Ltd., who manufacture a wide range of plastics materials, have a vacancy in the physics section of their development department for a Physicist (aged 25-35) to work on the electrical and mechanical properties of a wide variety of resins and allied products. Previous experience in this field is desirable but not essential, a more important requirement being a good honours degree in physics. Salary according to age and experience.

Applications giving details of age, training and qualifications should be addressed to

Development Manager,
Bakelite Ltd., Redfern Road, Tyseley,
Birmingham, 11.

IMPERIAL CHEMICAL INDUSTRIES LTD.

Billingham Division

PHYSICISTS

LIGHT CURRENT ELECTRICAL ENGINEERS

Imperial Chemical Industries Ltd., Billingham Division, has several interesting vacancies for Physicists. The Division has an expanding research programme and requires physicists to carry out work on the development of physical methods and techniques and their application to chemical problems and analytical methods.
There is also a requirement for Physicists and Light Current Electrical Engineers who have made a broad study of instrument technology or who wish to take up work in the fields of instrument development and management.
Candidates should be not more than 32 years of age and hold an honours degree. These appointments are pensionable and there is a profit sharing scheme in operation. Starting salaries will depend upon qualifications and experience. Write giving brief details of age, qualifications and experience to the Staff Manager, Imperial Chemical Industries Ltd., Billingham Division, Billingham, Co. Durham, quoting reference W/P/4.

PRINCIPAL SCIENTIFIC OFFICER required by the Atomic Weapons Research Establishment, Aldermaston, Berks., for planning, including arrangements for layout and scientific measurements, with eventual responsibility for the execution of atomic weapon trials. Some overseas duty involved. Candidates should hold a first or second class honours degree in physics and have a broad appreciation of atomic weapon effects. A wide experience of scientific measurements essential and some experience of field trials and of liaison between Civil and Service departments desirable.
Salary £1,390–£1,980 p.a. (male). Contributory superannuation scheme. A house or assistance towards legal expenses on house purchase will be available for married officers living beyond daily travelling distance.
Postcards for application forms to the Senior Recruitment Officer at the above address. Please quote ref. 1525/110.

A vacancy exists on the staff of The Physical Society for a graduate to undertake partly editorial and partly secretarial duties. The editorial side of the work concerns the preparation of manuscripts for the printers, the reading of proofs, etc. The secretarial side involves the checking of Membership records, liaison with the Group Officers, preparation of notices and Bulletins and programmes of meetings, and superintending their distribution. The starting salary will be in the range of £450 to £550 rising by £25 to £650. Applications should be made to the Secretary-Editor. Any students who will be graduating this summer are welcome to apply.

PILKINGTON BROTHERS LTD., who are the largest manufacturers of plate and sheet glass in the British Commonwealth and employ more than 20,000 persons, wish to appoint a Head of their Technical Development Department. This is a new appointment necessitated by the large number of development projects in hand. It is intended that the man selected should take charge of the development of new processes and the radical improvement of existing ones. There are ample technical resources available and the Company spends large amounts on development work. Applicants must have a good honours degree in engineering, physics or chemistry. They must also have a flair for development work, and the enthusiasm to see a job through. Applications will be welcomed from men aged preferably 35/45 who have done at least five years' successful and original development work in industry or at a university. The Company is prepared to offer a Service Agreement at an attractive salary to the right man.
All applications will be treated confidentially and should be addressed to the Group Personnel Officer, Pilkington Brothers Ltd., The Glass Works, St. Helens, Lancashire.

BRITISH PLASTER BOARD (Holdings) LTD., East Leake, Mr. Loughborough, Leics., need a physicist, preferably of Ph.D. standard for basic research and development of technological problems; some experience with dielectric properties of solids and/or properties of electrolytes an advantage. The laboratories, which are new, are situated in East Leake, equally convenient for Nottingham, or Loughborough. Salaries in accordance with age, qualifications and experience. Non-Contributory staff pension scheme. Write for form of application to Controller of Research quoting Tech. Phys/KS.

IMPERIAL CHEMICAL INDUSTRIES LTD., has a vacancy for an Engineer or Physicist to undertake liaison work on industrial instruments and control devices. Candidates must have a good honours degree and should be not more than 30 years of age. Previous industrial experience of instrument work would be an advantage. The successful applicant will be required to work in the first instance in the Company's Head Office in London but there will be the opportunity of transfer in two or three years time to research or management work in one of I.C.I.'s manufacturing divisions. Appointment carries membership of the Company's staff pension fund and employees' profit-sharing scheme. Applications, with brief details of age, qualifications and experience should be made to Head Office and Regions Staff Department, (GDC), Imperial Chemical House, Millbank, S.W.1.

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PHYSICISTS

I.C.I.

GENERAL CHEMICALS DIVISION

has vacancies for graduate physicists. The Division is building a number of new plants, the instrumentation and control of which present many complex problems of interest to physicists.

Particular attention is being paid to the development of equipment for analysing continuously the various products manufactured, and there are opportunities for research and development in many branches of physics including thermal conductivity, spectroscopy, optical absorption and electrochemistry.

The theoretical and practical aspects of the automatic control of chemical plants are being studied and this work, which will be of particular interest to those physicists who have an inclination to the mathematical side, is expected to lead to the design of still more advanced automatically-controlled plants.

There are opportunities for progressive careers in the fields of instrument design and maintenance as well as in research and development.

The vacancies are in the Runcorn, Cheshire, area and if necessary suitable candidates will have the opportunity of serving a short graduate apprenticeship to initiate them into the chemical industry.

Attractive starting salaries will be offered and the positions, which are on the established staff, will carry membership of the Staff Pension Fund. There is also an Employees' Profit Sharing Scheme and, if needed, assistance can be provided to married men towards house purchase and removal expenses.

Applicants should write to :

Staff Manager, Imperial Chemical Industries Limited,

General Chemicals Division, Cunard Building, Liverpool, 3

quoting reference IPH24